This is a digital copy of a book that was preserved for generations on library shelves before it was carefully scanned by Google as part of a project to make the world's books discoverable online.

It has survived long enough for the copyright to expire and the book to enter the public domain. A public domain book is one that was never subject to copyright or whose legal copyright term has expired. Whether a book is in the public domain may vary country to country. Public domain books are our gateways to the past, representing a wealth of history, culture and knowledge that's often difficult to discover.

Marks, notations and other marginalia present in the original volume will appear in this file - a reminder of this book's long journey from the publisher to a library and finally to you.

Usage guidelines

Google is proud to partner with libraries to digitize public domain materials and make them widely accessible. Public domain books belong to the public and we are merely their custodians. Nevertheless, this work is expensive, so in order to keep providing this resource, we have taken steps to prevent abuse by commercial parties, including placing technical restrictions on automated querying.

We also ask that you:

+ *Make non-commercial use of the files* We designed Google Book Search for use by individuals, and we request that you use these files for personal, non-commercial purposes.
+ *Refrain from automated querying* Do not send automated queries of any sort to Google's system: If you are conducting research on machine translation, optical character recognition or other areas where access to a large amount of text is helpful, please contact us. We encourage the use of public domain materials for these purposes and may be able to help.
+ *Maintain attribution* The Google "watermark" you see on each file is essential for informing people about this project and helping them find additional materials through Google Book Search. Please do not remove it.
+ *Keep it legal* Whatever your use, remember that you are responsible for ensuring that what you are doing is legal. Do not assume that just because we believe a book is in the public domain for users in the United States, that the work is also in the public domain for users in other countries. Whether a book is still in copyright varies from country to country, and we can't offer guidance on whether any specific use of any specific book is allowed. Please do not assume that a book's appearance in Google Book Search means it can be used in any manner anywhere in the world. Copyright infringement liability can be quite severe.

About Google Book Search

Google's mission is to organize the world's information and to make it universally accessible and useful. Google Book Search helps readers discover the world's books while helping authors and publishers reach new audiences. You can search through the full text of this book on the web at http://books.google.com/

ANNEX

[redacted]

A

JOURNAL

OF

NATURAL PHILOSOPHY,

CHEMISTRY,

AND

THE ARTS.

ASTOR LIBRARY NEW-YORK

VOL. XXIX.

Illustrated with Engravings.

BY WILLIAM NICHOLSON.

LONDON:
PRINTED BY W. STRATFORD, CROWN COURT, TEMPLE BAR, FOR
W. NICHOLSON,
No. 13, BLOOMSBURY SQUARE;
AND SOLD BY
J. STRATFORD, No. 112, HOLBORN HILL.

1811.

PREFACE.

THE Authors of Original Papers and Communications in the present Volume are Mrs. Agnes Ibbetson; J. D. Maycock, Esq.; Mr. G. J. Singer; Mr. E. Lydiatt; Mr. John Davy; W. Crane, Esq. F. R. M. S. Ed.; Mr. R. Lyall, M. R. P. S. E. &c.; R. L. Edgeworth, Esq. F. R. S. M. R. I. A.; L. O. C.; T. A. Knight, Esq. F. R. S. &c.; Mr. J. Dalton; the Rev. J. Blanchard; J. Clarke, M. D.; Mr. J. T. Price; Mr. St. Amand; T. Forster, Esq.; Mr. J. Murray; Marshall Hall, Esq.; W. Moore, Esq.; Mathematicus; Mr. B. Cook; W. N.; Zeno.

Of Foreign Works, Messrs. Gay-Lussac and Thenard; M. Regnier; M. Van Meerten; M. Stratingh; M. Cagniard Latour; M. de la Chabeaussiere; Mr. G. B. Sage; M. Bucholz; M. Daubuisson; M. Cordier; Dr. B. H. Tarry; M. C. Duméril; M. L. Cordier; M. Hassenfratz.

And of British Memoirs abridged or extracted, T. Thomson, M. D. F. R. S. E. &c.; the Rev. J. Bremner; T. A. Knight, Esq. F. R. S. &c.; H. Davy, Esq. LL. D. Sec. R. S. Prof. Chem. R. I. F. R. S. E.; Mr. E. Smith; Mr. J. Hutton, junr.; H. P. Lee, Esq.; the Rev. J. Hall; Mr. T. Balls; Mr. J. Baker; Mr. W. Jeffery; Mr. J. Davis; Mr. W. Moult; Mr. B. Smith; Mr. J. Taylor; B. C. Brodie, Esq. F. R. S.

The Engravings consist of 1. Dissections of Plants, illustrating the Growth of the Bud, by Mrs. A. Ibbetson. 2 Crystals of Allanite, by Dr. T. Thompson. 3. Instruments for measuring the Velocity of Rivers, by Mr. Regnier. 4. Method of covering a Roof with Flagstones, by R. L. Edgeworth, Esq. F. R. S. M. R. I. A. 5. A Ship's ordinary Boats converted into Lifeboats, by the Rev. J. Bremner. 6. A Barometer with an adjusting Scale, and 7. An Airpump for producing a perfect Vacuum, by a Correspondent. 8. Section of a Grapehouse, by T. A. Knight, Esq. F. R. L. and H. SS. 9. Figures to illustrate the Formation of the Leaves of Firs, and their Fructification, and the Motion of the Flower of the Barberry; delineated by Mrs. Agnes Ibbetson. 10. A Diagram illustrating the Radiation of Cold, by Marshall Hall, Esq. 11. Diagrams illustrative of the Motion of Rockets, by W. Moore, Esq. 12. A new Thrashing Machine, invented by H. P. Lee, Esq. 13. A Screw adjusting Plough, by Mr. Thomas Balls. 14. An improved Implement for extirpating Docks and Thistles, by Mr. J. Baker. 15. A pair of expanding Harrows, by Mr. Wm. Jeffery. 16. Mr. J. Davis's Fire-escape. 17. Mr. W. Moult's Filtering Apparatus. 18. Mr. B. Smith's Method of Relieving a Horfe, that has fallen in the Shafts of a loaded Cart. 19. Mr. J. Taylor's Extractor of foul Air from Mines, &c.

TABLE OF CONTENTS

TO THIS TWENTY-NINTH VOLUME.

MAY, 1811.

JUNE,

JUNE, 1811.

JULY, 1811.

AUGUST, 1811.

SUPPLEMENT TO VOL. XXIX.

A
JOURNAL
OF
NATURAL PHILOSOPHY, CHEMISTRY,
AND
THE ARTS.

MAY, 1811.

ARTICLE I.

On the Interior of Plants. Letter II. By Mrs. Agnes Ibbetson.

To Mr. NICHOLSON.

SIR,

I Shall now give a regular history of buds, and their manner of growing, as it has been hinted to me, that the sketch I gave in my last was not sufficiently explanatory and ample, considering the importance of the subject to botany, its novelty, and how little the real formation of the interior of plants is understood. It is certain, that the diligence of gardeners has far exceeded the labour of physiologists in this respect, and established first from accident, and then from practical experience, many rules, which should have been suggested and taught by the philosophers of botany; but I believe the scientific part of this science seldom travels as fast as the practical, and that it is usually left for the former to account for the reason why the process is good or bad, after it has been thoroughly established. But this may not always be the case; when once we have a thorough knowledge of the "interior formation of plants", the scientific

others. The whole plant is formed of leaves, till flowering time, except the root, (which in every respect agrees with other plants, having the same divisions between the root and leaves, as I discovered between the root and stem in other plants, and which I called the grand obstruction): but when the time arrives for the plants to become prolific, there runs up from the root a slender thread, at first like packthread, within the axil of the leaf, and under the cuticle. This by degrees increases. It is composed of many little parcels of the germe of buds, in each of which is the knot of the line of life. As they rise, they enlarge, till, too much swelled for confinement, they burst forth into flowers; appearing to grow from the leaf: but they have in reality no connexion with it, except that of borrowing from it the spiral and nourishing vessels, which run into the corolla. I have traced the palm when just going to flower: for though from the want of air in the hothouse, it had never flowered, yet the buds were within. I found more in the root just ready to run up, and some half way; it is exactly made like the grasses, and like the arums, and every plant of that kind which has no stem; but in palms it is impossible to know all this without dissecting one. This order of plants flowers in various ways, but generally, at the top of the plant. The grasses carry up their diminutive buds through the flower stalk, one by one, with the line of life. In the arums it is very easy to trace the buds, but they increase more and in a quicker manner than in most of this class.

4th sort of bud. My fourth sort of bud is confined within a bulbous root. This explains itself; it begins at the root, and completes its form, more than is usual in any other plant, except those of the ranunculus tribe. Before it leaves the root all its parts are generally designed; they only enlarge, and the peduncle grows and raises them to view, having the bud at the top, hence the historics of the flowers to be found within the bulb. There are many however, that by no means resemble what they are to be, any more than many germes, when first concealed in the bud; because the parts are yet seldom proportioned to each other. The tulip and hyacinth are peculiarly perfect in the bulb.

I have now given a simple account, sufficient to make all understand

understand the nature and progress of buds in all plants, except the cryptogamia and water-plants. These I am deeply studying; and I flatter myself it will not be long before I shall be able to complete my sketch in this respect.—I shall now endeavour to account for the cause of the succeeding of various means lately adopted in gardening, and show the reason that success attends such practices. All that Forsyth did to old trees, was to take from them the rotten part, which wholly checked the growth of the albumen, by soaking up the juices, which should have produced it: but the sap once returned to its usual office, forming the new wood, or albumen, gave an increased vigour to the line of life, which, when the rotten part was cut away, had room to shoot afresh, and by a quicker circulation of the sap renewed the vigour of every part of the plant. Scarcely has the year power to run its usual circle, before a tree so reanimated will begin to shoot at the very heart; the little pith to be found in so old a tree can hardly raise moisture enough for the innumerable buds, which form in every part, on the line of life; beginning at the dilapidated part, and soon communicating to all the rest. It is astonishing what good may be done, by thus now and then paring away a part, that appears to be beginning to decay; but then it must be cut with great care, and covered with the plaster ordered by Forsyth, which is excellent; and not left to contract the rot. The dried stump of a tree, or the remains of a broken limb, may in a very few years (by this management) be the source of new shoots, more than equal to those before lost; and though I believe with that philosophic observer Mr. Knight, that there is a term of life and vigour, beyond which a tree cannot pass: yet it is certain, that it is a very ancient time, and that almost all our trees die from careless inattention, and probably at half their proper age.

New practices in gardening.

Why Forsyth's method succeeds.

The preservative to the wound.

There is a strange idea universally spread among physiologists, arising (I must think) from our ignorance of the interior formation of plants, but universally received as a truth; that as wood grows old, it contracts in form, till all the passages of the fluids are stopped, and it remains in a kind of torpid state, till it dies. This idea appears to me to be so contradicted by all I have seen in nature, by my hourly

Sap in a tree never stagnates.

hourly study of the disorders in plants, and by the consideration of that sort of life which plants possess, that I am very anxious to show the fallacy of it. Wood once reduced to this state (it stands to reason) could never again recover; it could never throw out buds, it could never again be restored to a regular rising of the sap; for the vessels are so small, that, once choked, it would require a miracle indeed to open them; and yet it is well known, that a tree may be restored to almost pristine vigour, by a little cutting and care: and then, so far from being in a fixed state, every vessel of the wood must be moved out of its place, must bend in one way or other for the exit of the buds, the juices must be so plentiful (the sap in particular) as to form albumen to engender and accompany all those buds. Where then is its torpidity? It is true, that, the older wood grows, the more it is compressed; but it is the middle part between the vessels, that is reduced. A very simple proof may be given of this, by cutting the oldest piece of wood, that can be found, in a living tree, and placing it in the fire: the quantity of sap, which runs from each separate vessel, bubbling and spouting out as soon as the heat acts, will quickly show how full of sap the oldest vessels are. But this very compression only more strongly proves the health and strength of the tree; it quickens the circulation of the juices by pressing the bastard pipes against the sap vessels, and thus gives increased vigour to the tree. I will be bound to say, that the passage of the vessels was never suspended in a plant, without causing the gangrene directly, and very soon death*. To prove, that I am not too hasty in this assertion, I will simply show the general manner of the death of trees, when they die naturally, and without accident. The first appearance of sickness is the hanging down of the branches and leaves: this is followed by a sweetness pervading all the different juices of the plant, attracting every species of insect, which soon cover and spoil the leaves with their filth: then little divisions of the wood (grown weaker than the rest) burst their vessels, and begin a sort of rot, which increases daily: the spiral wires, which attach the leaves to the stem, begin

Parts between the vessels compressed by health.

Death of a tree.

* This is so peculiarly the case; that almost all the disorders of trees arise from a stoppage of the circulation in different parts.

to

to grow brittle, and their cases to crack: the nourishing vessels round them decay, and the first wind, of course, takes off the leaves, and the next circumstance is generally the death of the line of life; which, when once it begins to be affected, soon bursts, turns black and dies: this spreads an increased sweetness over the plant, by the juices of the line; which, though often bitter, are luscious, and tempt the worm. From this time nothing can save the tree, though the bark and rind may still show some green; nay, I have known a fine day burst the leaf-buds, so little has the leaf to do with the plant, but they are soon gone; and the remainder sinks to torpidity and death. I have watched many trees from the first to the last in this way, and taken down their symptoms as they increased, by cutting branches, and thus judging of the progress of the evil; but if at the first appearance of it, care had been taken, the tree had been dug round, and a little dressing thrown with fresh earth; and if the disorder continued, and showed in any particular spot; had this been cut away, and managed as mentioned above, for to excite to fresh action is every thing in a plant, and air and light if possible let to it by cutting down any rubbish that impeded it, many trees might be saved, and much wood restored to the country. Light is certainly the most necessary desideratum to plants. It is painful to see how trees will twist their branches in search of it, and perhaps be disappointed at last. A tree is therefore so far from dying by too much compression, that this is always a sign of health; as the spreading out and growing irregular in the branches is a sign of sickness. But I must dwell on this subject no longer.

To excite to action gives health to a plant.

I mentioned, that when a bud is protruded, a knot is formed on the line of life, which is broken, and the two ends form buds. All that is necessary therefore to form a bud is, to divide the line of life; this gardeners have learned to do, by cutting a gash in the place they mean to make prolific. They then not only divide the line, but they also separate the wood, which hastens the bud, as it has not to prepare the wood for its exit from the plant. This very much quickens the business; but then there is evident danger in the doing it. In the first place many buds may be destroyed in their way;

The breaking of the line of life,

way: the finger should therefore be pressed all up the part, to be well assured that there is no branch on the point of shooting; the bud will be felt as soon as the bark and rind have made a socket or bed for its reception, which is done before the bud is half way on its journey: then a plaster should be prepared to cover the gash, without pressing it too close, but taking care to guard it well from the air, lest any should get in and cause a rot, more easily gained than cured. I have often found a bit of bladder, placed under the plaster, a better preservative than any thing else, if perfectly clean, and free from all grease.

forms buds.

Difference of the flower bud and mixed bud.

I promised at the conclusion of this letter to show the difference of the flower bud, and the leaf and flower bud, which is very trifling. They both come from the same place—the line of life—and both in the same manner. In the mixed bud, when arrived at its cradle, the rudiment of the flower stops while the leaf is weaving. The first has also some few leaves to complete, and many scales. The female or pistil of both was a rude mass containing the seeds, but now begins to take its proper form; while the males, all joined together, and proceeding from the wood, are completely fashioned. The scales in the mean time are growing to cover it thoroughly, and most buds have a quantity of their juices (that is the blood of the plant) lying between the several covers, as a sort of resin, to protect it from the air and cold, of which it is now very susceptible. In the mixed bud, the leaves always are finished at the top, before the flower, even where the flower comes out first, to prevent the matter of the leaf, or mixing with the juices of the flower; a care which is peculiarly evinced throughout the whole formation of plants; and which it is wonderful to me physiologists have not observed, since their whole make is founded on this principle—the keeping all their juices perfectly separate. For this reason all the vegetable world is formed cylinder within cylinder; and, when there are holes, they are so contrived, that nothing but air can enter them. I shall soon exemplify this by delineations of the passage between the stem and the peduncle; which plainly show how strongly this principle is maintained in every instance, and how little therefore we can judge of the effect of the juices

Various juices of the tree always keep separate.

juices when we mix them all together. As to the leaf bud, I have in my last letter said, it is begun and finished in the bark. It is indeed a history in itself, and one of the most wonderful I know. There is so much pressing, rolling, and weaving, that I have constantly viewed it with fresh astonishment; for after being woven with all its parts loose and open, and all the ends hanging to it, like a piece of cloth fresh from the loom, it is folded anew, rolled in a particular manner, and laid in a liquid; then unrolled, and again folded in another manner, and pressed in the bud; and this is repeated several times, till by degrees losing all its ends, it is prepared for making the edges, which is the most curious part of all. I have already detailed this in my first letter, and shall not therefore repeat it, but only say, that the leaf buds of those plants, which have no stem, are formed within the bosom of the other leaves, joined to one end of the cuticle, not in the root. I have much to say on this subject, but it must be in another letter, and one which is restrained to leaves alone.

Formations of the leaf bud.

I must now say a few words on a subject I have long deferred touching upon, but which I have not the less studied; indeed I hardly know one that has lately engrossed so much of my thoughts; I mean, "whether there is, or is not, a circulation of sap through plants." After the most mature inquiry, the most exact research, I cannot discover the slightest reason for believing, that it takes place even in trees; on the contrary, the most potent arguments, drawn from the very nature of the vegetable tribe, militate against it. That there is a regular passage upwards for the rise of the sap, no one denies; but returning vessels from the head of the plant to the root I must think a fallacy; arising from that unfortunate comparison established between the animal and vegetable world, which was well enough in the first birth of both, but has been carried in my opinion to a false and blamable extent. Can any thing be more unlike animal life, than the shooting of the buds? This will, I think, more plainly appear, in drawing a comparison between the functions of both —in an animal constant motion is necessary to circulate the blood; its juices, formed in the body itself, from the different secretions I believe, (but I do not understand anatomy,)

Whether there are returning vessels for the sap or not?

Sap too much exhausted by its various formations.

tomy,) and constantly added to by food as solid as the flesh it creates, and as the blood it produces. There is no yearly extension of body, except a trifling increase at first, that could require the absorption of such a fund of matter as the whole blood of the animal to produce it: but in a tree each year creates almost a fifth of the weight, unless the tree is very large, in fruit, flowers, leaves, fresh branches, new radicles, and seeds. Whence then could the returning juices flow, exhausted as they must be? I have weighed the yearly increase in a small tree, and it far exceeded this calculation. Besides in an animal the blood is formed in it, whereas in a tree the sap is the juices of the earth. Nature would not therefore draw up more than is necessary for its various productions, merely to carry it down again. In animals the circulation, increased by exercise, occasions a constant dissipation of the several parts, which enter into its composition, and is therefore, I understand, productive of a thousand good consequences, without which the animal might become torpid and insensible, from the effects produced on the brain; but in a tree I see no end it can answer; nor could I ever find any returning vessels that would carry coloured juices the contrary way, though I have sought them in every part of the plant. As to the reason given, "that, if a deep piece of wood was cut out of a tree, a large portion of matter grew in the upper part of the wound, and none in the lower," it is easily to be accounted for. The moment a tree grows unhealthy, it gets full of these bunches; but such a cut must at once produce them. The first effect of such a dilapidation is to arrest the vigorous flow of the sap: much of this is therefore stopped, and often breaks some of the wood vessels: this forms little pools, which occasion the rot, while the other vessels, filled with air, are inflated and increased: in the mean time the line of life, which has been divided by the deep cut, shoots out many new germes, and forms new wood to engender them; and when you take off the lump so made, it is a mass of loose wood, of rotten albumen, and new shoots half alive, and half dead; while the under part, losing its sap by bleeding, which the other could not do, as the vessels could not discharge themselves backwards, is only dried up; and the buds, not being able to form for want of the sap, decay in their first

Wrongly accounted for.

first shooting, and of course the lower part of the wound is not at all increased in size. This appears to me perfectly the truth. I have dissected many pieces grown in this way, and they have always proved such as I have described above; it is therefore a proof which militates against the return of the sap vessels, rather than for it. The manner of the forming of the bud is also much against it, and I know not a single reason for it. Perfectly to understand the formation of the juices and to be able to separate their different parts and analyze them as they should be, is at present my most anxious wish. There are in all plants four different sorts of juices, which should with the greatest care be kept asunder; I have some curious details in this respect, though scarcely yet worthy to be laid before the public; but I hope my next experiments will be more exact, however I procured some very curious crystals of a peculiar shape, by means of subjecting the juices of the line of life to a very strong heat, and afterwards cooling it very gently; but I hope to procure better information; for I am but an indifferent chemist, though often dabbling in the science.

Four different sorts of juices in plants.

Though I have not in my last two letters taken notice of the foundation on which my studies rest, that for which I principally undertook to give the dissection of the interior of plants, that which appears to me to be the fundamental and systematic part of botany, "the natural affinity of plants to each other," yet I have not forgotten it. I continue to pursue it with the most exact care, noticing with attention each connexion; and strong lines indeed does the formation of buds draw between plant and plant, as I shall soon show; encouraging my hopes of finding something like a plan, on which a system may be discovered, without expanding into rules too diffuse to serve such a purpose. That the greatest, the most scientific botanist will ever be able to make one generally useful, and to supersede all artificial methods, I much doubt, when such a master as Jussieu has failed, but that one of more humble pretensions may be found; I am still most sanguine in my hopes, since the more I dissect the more proofs of that system I find in nature.

Natural affinity of plants.

I am, Sir, your obliged humble servant,

AGNES IBBETSON.

Explanation

Explanation of the Plate.

Plate I, fig. 1, A horizontal cutting of the red cabbage; to show the difference between the leaf bud and flower bud in annuals: the flower bud proceeding from the line of life, flowing within the pith, as at *a a*; the leaf bud generated within the rest of the leaves, as at *b b b*.

Fig. 2, The interior of the leaf bud, where many flowers grow in a bunch; the scales taken off. *c c c* new flower buds just generated on the line of life.

Fig. 3, A leaf bud, where no line of life is to be found.

Fig. 4, Mixed bud, of the apricot, in which the flower is completely separated from the leaf. *d*, the female; *e*, males; *f*, the line of life.

Fig. 5, The manner in which the leaf buds grow in the palmate buds. Each spiral turn makes a separate bud.

Fig. 6, A cutting of the potentilla; showing the circular line of albumen, in which the buds are formed. I have since found a vast number of annuals formed in this manner; that is, having the circular line of albumen within the line of the pith, in which the buds are very much formed.

Fig. 7, A sort of screws formed at the end of many new shoots, which are cradles for buds.

II.

Observations on the Hypothesis, which refers chemical Affinity to the electrical Energies of the Particles of Matter. By J. D. MAYCOCK, *Esq. Communicated by the Author**.

Mr. Davy's hypothesis of

Sect. I. FROM the consideration of an important, and interesting series of phenomena†, Mr. Davy has thrown on

* This Essay, in very nearly its present form, was read to the Royal Medical Society of Edinburgh, on the 13th of March, in answer to a question proposed by the Society, and gained the gold medal. The question

† Phil. Trans. 1807: or Journ. vol. XVIII, p. 321, XIX, p. 37.

out a conjecture, that chemical affinity and electrical attraction are identical forces; and has very ingeniously endeavoured to point out the general application of the principle. This hypothesis, proposed by its author in the form of a question, has been too hastily admitted by some as an established doctrine; and speculations have been founded on it, which lead to the most extensive and unexpected conclusions. chemical affinity.

The precipitate adoption of this hypothesis seems to have arisen principally from the imperfect and confused notions commonly prevailing in respect to electrical phenomena. I therefore deem it necessary, before proceeding to discuss the question proposed by Mr. Davy, to state such of the principal phenomena of electricity, as may unequivocally define in what sense we are to understand the terms electrical state, and electrical energy—terms which will often occur in the following pages; and which are to be esteemed synonimous; both being employed to denote a certain state of existence of bodies, in which peculiar phenomena are evinced. Electrical state and electrical energy the same.

Bodies are said to be in different electrical states, or to have dissimilar electrical energies, when they attract each other: their electrical states or energies are said to be similar, when they repel each other. But we are to keep in mind, that these electrical attractions and repulsions are, in their effects, distinct from the attractions and repulsions, which bodies ordinarily evince. Two cork balls, suspended by silk lines, will indicate attraction or repulsion, accordingly as they may be in different or similar electrical states; and in either case, the motions arising in the balls will be in direct opposition to such as take place in consequence of the law of gravitation:—they will be diametrically contrary to those which appear in the action of the pendulum. Different and similar states. Actions produced by these.

question was: "Whether are the phenomena produced in the decomposition of bodies by galvanism capable of being explained on the usual principles of chemical attrac ion; or do they seem to establish the theory, that chemical phenomena depend entirely on the electrical energies of the particles of matter?"

When

When bodies are attracted, in consequence of a difference in their electrical states, and come into contact, or within a certain degree of proximity, each of them acquires a new electrical state, and the new electrical states are found to be similar: for between the bodies there is now exerted a repellent force. The operation, by which difference of electrical state is destroyed, is very frequently attended by the emission of light, a crackling noise, a peculiar smell, &c. The property, by which a body is brought to the same electrical state as that of surrounding bodies, is termed the conducting power, and is very various in different substances. Metals have the greatest, sealing wax and glass the least conducting force.

Vary in their force.

Bodies may, on account of their electrical states, attract or repel each other with various degrees of force; we therefore conclude, that various degrees of difference in the electrical states of attracting bodies exist; and that the electrical states of repellent bodies vary in different degrees from the electrical state of surrounding bodies.

The same operation produces opposite electrical effects.

When two dissimilar bodies are subjected to the same operation, the electrical state produced in the one is more or less different from that excited in the other. The same operation, indeed, not unfrequently appears to be the cause of diametrically opposite effects, when applied to dissimilar bodies. If a glass rod, and a rod of sealing wax, be excited by friction, and their electrical states be communicated to two insulated balls, which may be represented by the signs A and B: both these balls will exert an attractive force on the surrounding bodies: but A will more powerfully attract those bodies, which have been in contact with B; and *vice versa* B those, which have been in contact with A, than those which remain in their natural state. From this fact we learn, that the sealing wax and the glass differ less from surrounding bodies, in their electrical states, than they do, in this respect, from one another; and consequently, that the friction had produced opposite effects on them. In the theory of Dr. Franklin, an electric fluid is supposed to be accumulated in the glass, and dissipated in the sealing wax. Admitting the existence of an electric fluid, it would seem to follow, that, if it be accumulated in the glass,

Plus and minus electricity.

glass, it must be dissipated in the sealing wax: but as far as my knowledge goes, it has never been determined, that it is in the glass, and not in the sealing wax, that the accumulation takes place. I mention so much of the theory of Dr. Franklin, not with an intention of entering into a defence or refutation of its principles, but rather to point out the origin of the terms positive and negative, plus and minus, as applied to the electrical states of bodies. I continue to employ these expressions, as it would be difficult at present to invent others freer than they are from hypothesis.

A body, to show signs of electricity, should be near another in a different state.

It is important to remark, that the phenomena, which have been enumerated, do not occur in every electrified body. That signs of electricity be evinced, it is essentially requisite, that the electrified body be in a state of proximity with other bodies electrified in a different manner.—I insulated one of Bennet's electrometers*, and, by a bent wire, connected the foot and the plate of the instrument. When I electrified this wire, a momentary and extremely trifling effect was produced on the gold leaves; but they returned to their natural position, although the whole apparatus was kept by one experiment in a state highly positive, by another in a state highly negative. The repulsion, when duly established, appeared to be equal between the gold leaves, and between the gold leaves and the tin foil. Had either the gold leaves or the tin foil been alone electrified, the effect, as is well known, would have been a separation of the gold leaves. From this experiment it also appears, that our Earth may *possibly* be very highly positive, or very highly negative, in relation to any other of the planets, without our instruments indicating such state; our Earth bearing the same relation to the bodies of the universe, that an insulated electrometer does to the various other bodies of our Earth. That the various bodies of our Earth do naturally possess the quality we denominate electrical, is an opinion, not only probable from many general considerations, but one which admits of proof from the following statement

The various bodies of our Earth naturally possess electricity.

* I use the electrometer improved by Mr. Cuthbertson, but without the condenser.

statement of fact. If two metallic balls, A and B, be placed near to each other, and to a small cork ball, suspended by silk, and positively or negatively electrified, which may be called C; and if A be connected with the Earth, and B be positively or negatively electrified in a greater degree than C; A and B will both attract C: but A will attract C with greater force after it has been in contact with B, than before; and the contact of C with A will augment the attraction between C and B. The effect is precisely the same as would have arisen, had A and B been both insulated, and differently electrified, and C connected with the Earth.

Circumstances may prevent the appearance of attraction.

It is also to be remarked, that, although two bodies, in different electrical states, be near to each other, it is very possible, that they may not indicate attraction. If, for instance, two fixed and insulated metallic balls be electrified, the one positively, the other negatively, and a small bit of cork, suspended by silk, be brought between them, the attraction of the cork for one metallic ball may be just sufficient to counteract its attraction for the other.

The preceding observations are unconnected with any hypothesis concerning the remote cause of electrical phenomena; and are, indeed, nothing more than a general statement of facts, established by experiment. Electricity is therefore a science, which has for its object phenomena produced in consequence of a difference in the electrical state of bodies, so situate, as to be within the sphere of action of each other; among which phenomena are certain modifications of the attractive and repulsive forces, that bodies ordinarily evince. Electrical state, or electrical energy, is the quality, to which such phenomena are referred. These conclusions are obviously deduced from the artificial electrical states; but, if they do not equally apply to the natural electrical states of bodies, I confess I have no idea of what is meant by this expression.

That difference of electrical state is the same with chemical affinity requires proof.

Sect. II. From a consideration of electrical phenomena in general, but more particularly of those which occur during decomposition by galvanism, Mr. Davy thinks it probable, that difference of electrical state is identical with chemical affinity, and an essential property of matter.

ter*. The important effects that such a principle, if adopted, would have on our chemical and physical reasonings, certainly require, that it should be established by the most satisfactory evidence. How far it is so will appear from the following observations and experiments.

Connexion of chemical action with gravitation,

Gravitation and chemical union are operations apparently dissimilar, and it is by no means surprising, that they should have been, for a considerable time, referred to the agency of different powers. At present we cannot but perceive, that gravitation is intimately connected with chemical action, by the various intermediate effects of the attraction of cohesion, of capillary attraction, and of hygrometric affinity. It has never, indeed, been demonstrated, that chemical affinity is identical with the attraction of gravitation; nor do I consider the opinion as admitting of such proof. Philosophy has in this instance done enough, and perhaps its utmost, in removing all objections to a general doctrine, which is recommended by strong and insuperable analogies. Now those who admit, that gravitation and chemical affinity depend on the same principle, cannot for a moment maintain, that chemical attraction and electrical attraction are identical: for it can be demonstrated, that the attraction of gravitation is not identical with electrical attraction.

which is not the same with electrical attraction,

for bodies in different electrical states do not gravitate differently to the centre;

In the first place, if gravitation depend on difference of electrical state, there must be some body at the centre of the Earth having an electrical state different from that of every body at the surface, since every body at the surface is apparently attracted to the centre. But as all bodies at the surface are supposed to have different electrical states, in respect to one another, there cannot exist the same degree of difference between the electrical state of any two dissimilar bodies, and that of the body at the centre, and consequently dissimilar bodies should be attracted to the centre with unequal degrees of force: a conclusion perfectly inconsistent with the principles established by Sir Isaac Newton's beautiful experiments with the pendulum,

* Philos. Trans. 1807, p 29: or Jour. vol. XIX, p. 80.

tion*, and on which is founded the whole system of natural philosophy.

and electrical attraction is proportional to the surface, gravitation to the mass.

In the second place the force of electrical attraction is, *cæteris paribus*, proportionate to the extent of the surfaces of the attracting bodies; gravitation, on the contrary, is proportionate to the quantity of matter reciprocally attracting, and has no dependance on the extent of surface. This essential difference between the two powers is peculiarly striking.—A bit of gold leaf, of tin foil, or of sheet lead, will acquire a rapid motion through the air, when acted on by an excited electric, which would not sensibly affect an equal quantity of either of these metals in a globular form; and yet the gold leaf, the tin foil, or the sheet lead will have to overcome considerably more resistance in passing through the air, than will a globule of gold, of tin, or of lead. On the contrary a given quantity of gold, of tin, or of lead, will gravitate more rapidly, in the medium of our atmosphere, when in a globular form, than when beaten out into gold leaf, tin foil, or sheet lead; not that the force of attraction is diminished by the extension of the metals, but because in an extended form they have to overcome a greater degree of resistance from the elastic medium, through which they are to pass.

Thus, I conceive, it is demonstratively proved, that the attraction of gravitation is not identical with electrical attraction. Our views, therefore, of the phenomena of nature would not be rendered more simple by admitting, that chemical attraction and electrical attraction are identical; as this would draw a line of distinction between the principle of chemical attraction, and the principle of gravitation. Every one must determine for himself whether the hypothesis, proposed by Mr. Davy, be supported by arguments more plausible, than those, which can be adduced in favour of the identity of chemical attraction, and the attraction of gravitation.

Attraction considered as

Philosophers of the present day most commonly speak of attraction as an ultimate property of matter; not that they

* Newtoni Phil. Nat. Princip. Math. L. III, Prop. VI, Theor. VI.

conceive

conceive it to be really so, but that they are unwilling to add to the many vain speculations, which have been proposed to account for it. What indeed has been denominated electrical attraction, of which Mr. Davy considers chemical affinity to be a modification, is yet very generally referred to the agency of a subtile, and essentially fluid body. Repulsion is almost universally attributed to the action of such fluids. It is however to be remarked, that we have no direct evidence of the existence of these fluids, as they have never been the objects of sense. They are agents erected entirely by human ingenuity, for the purpose of explaining phenomena, which in the pride of speculation we are unwilling to admit are inexplicable. The whole of the modern doctrines, respecting light, caloric, and the electric fluid, are hypothetical, and allow only of such indirect evidence, as is derived from their capability of explaining the class of phenomena, on account of which they were assumed. That they do so to a certain extent cannot be questioned. It must, however, be admitted by every one who patiently investigates the subject, that there are phenomena, connected with the temperature and electrical state of bodies, which cannot satisfactorily be accounted for on the generally received opinions; and although there is, at present, no positive objection to the supposition, that light is a material substance; such may possibly arise in the progress of discovery. The hypothesis, therefore, which refers repulsion, or any modification of attraction, to subtile fluids, although it need not altogether be rejected, should be received with caution, and never made the basis of our general principles.

an ultimate property of matter.

In the present state of our knowledge, it would, perhaps, be most prudent to abstain from all speculations, concerning the cause of attraction and repulsion, and to consider them both as properties of matter, prevailing under different circumstances. It is at least certain, that we have as unquestionable experience of the particles of ponderable matter repelling, as of their attracting each other. Now it cannot be doubted, that attraction and repulsion are very much modified and affected, among other causes, by those which modify and affect the electrical state of bodies. Indeed

Attraction and repulsion.

Modified by causes affecting electricity.

such

such modifications of attraction and repulsion are among the most obvious phenomena of electricity, and those that first gave origin to this science. But after what has been said, it must appear impossible to consider difference of electrical state as identical with the principle of attraction. Neither do I think it could be seriously contended, that similarity of electrical state is identical with the principle of repulsion; as this would, at least, involve the opinion, that similarity of electrical state, whether positive, or negative, is identical with the cause of increased temperature; an opinion I, by no means, feel myself called upon to confute.

Supposition of minute differences in the electrical state of dissimilar particles.

Possibly, however, it was never meant, that difference of electrical state is identical with the principle of chemical attraction; but that there exist minute differences in the electrical states of the particles of dissimilar kinds of matter,—that such differences are not destroyed by contact,—and that, although they are not sufficient sensibly to affect the vibrations of the pendulum, they may yet so modify the principle of attraction, as to give rise to the phenomena which favour the idea of *elective* affinity. This is certainly the least objectionable form the hypothesis can assume. The supposition, however, that dissimilar bodies preserve different electrical states, is in opposition to analogy; since we invariably perceive a tendency in bodies to acquire similar electrical states, as far, at least, as our most delicate instruments inform us; and as this law holds to every measurable difference, it would surely be unphilosophical not to consider it as absolute, and without exception.

Particles might have different capacities for electricity.

Were we to adopt the theory of Dr. Franklin, it might appear to follow from many facts, that dissimilar bodies have different capacities for the electric fluid; but this would surely afford no stronger argument in favour of the opinion, that bodies exist in different electrical states, than might be drawn from their having different capacities for caloric, in support of an opinion, that they have naturally different temperatures. The hypothesis proposed by Mr. Davy cannot, therefore, be admitted, until it shall have been proved, that it is capable of explaining, in the most satisfactory manner, the phenomena on account of which it was assumed; and that these phenomena are inexplicable on

any

any known, and less exceptionable principle. I shall endeavour to determine how far Mr. Davy's proposition brings with it these recommendations; and would here observe, that the following arguments will equally apply, whether it be held, that the principle of chemical attraction is identical with difference of electrical state, or, that the principle of chemical attraction is modified by difference of electrical state.

Certain substances said to be attracted by positive, others by negative electricity;

Mr. Davy's speculation rests entirely on the correctness of his position, relative to the "changes and transitions by electricity." He states, not as an hypothesis, but as a general expression of fact, that "hidrogen, the alkaline substances, the metals, and certain metallic oxides, are attracted by negatively electrified metallic surfaces; and repelled by positively electrified metallic surfaces; and contrariwise, that oxigen and acid substances are attracted by positively electrified metallic surfaces, and repelled by negatively electrified metallic surfaces; and that these attractive and repulsive forces are sufficiently energetic, to destroy or suspend the usual operation of elective affinity*.

but this not always the case.

To determine, whether Mr. Davy's statement be correct, I selected one, from each of the classes of substances enumerated in the preceding paragraph: viz. boracic acid, barytes, and gold-leaf, and I found, that the metal and the earth were attracted as powerfully by an insulated metallic ball, electrified by glass, as by the same ball, electrified by sealing-wax. I also satisfied myself by experiment, that the acid is indifferently attracted by a positively or a negatively electrified metallic surface. It is impossible to operate on oxigen and hidrogen in their uncombined state, and thus to determine the truth of Mr. Davy's statement, as it relates to these substances. This circumstance is, however, the less to be regretted, as, when analogies are so forcible, and so obvious, as in the present instance, the conclusions, which are drawn from them, are received by the mind with a degree of certainty, little inferior to that, which is derived from demonstration.

These facts

Simple as these experiments may appear, they are decidedly adverse to Mr. Davy's hypothesis, the essential and

* Phil. Trans. 1807, p. 28: Journal, vol. XIX, p. 41.

indispensible

adverse to the hypothesis.

indispensible principle of which is, that particular substances have certain natural preferences and aversions to positively and to negatively electrified metallic surfaces; as they prove, that no such preferences and aversions are evident, while the substances acted on by the electrified surfaces remain in their natural electrical state. We cannot, indeed, by any means, infer from the result of these experiments, that bodies do not exist in different states of electricity; but we must feel satisfied, that an acid is not repelled by a negatively electrified metallic surface, or an earth or metal by a positively electrified metallic surface;—positions which form a very principal part of Mr. Davy's hypothesis.

Peculiarities of chemical action may be accounted for by the hypothesis; but equally on other grounds.

A supposition, that dissimilar bodies exist naturally in different electrical states, may possibly enable us to account for many of the peculiarities of chemical action; but I am inclined to think, that these peculiarities are explicable without the supposition, and that the philosophical labours of Berthollet have pointed out, with sufficient accuracy, the circumstances, which modify the principle of attraction, when excited on the minute particles of matter. The question, at present under consideration, did not, however, originate in the phenomena of chemical affinity, but was rather suggested to Mr. Davy, by the electro-motive property of bodies, and the truly valuable discoveries which have lately been effected by means of galvanism.

Bodies being in different electrical states after separation no proof, that they were so before.

If, after the contact and subsequent separation of two dissimilar bodies, they are found to be in different electrical states, in respect to one another, and to surrounding bodies, to what they were in before the contact, can we infer from this, that they must necessarily have existed in different electrical states, in respect to one another, previously to the experiment? Surely not. The electrical states, they now possess, have evidently been produced by contact, or subsequent separation. It may, indeed, be difficult to perceive the connexion between the effect and its cause; but this cannot warrant us in supposing, that bodies are in different electrical states, when our most delicate instruments assure us, that they are in similar electrical states. Was it, indeed, granted to us, that dissimilar bodies have naturally different electrical states, we could not, on this principle, consistently explain

explain their electro-motive property; since we set out with supposing, that they retain their particular electrical states, although contiguous with conductors.

Let us now turn our attention to the phenomena produced during decomposition by galvanism; and in the first place let us inquire, whether they can be accounted for on the principles proposed by Mr. Davy;—in the second place whether they cannot be accounted for on principles less objectionable. Phenomena of galvanic decomposition.

Had it been proved, in the most unexceptionable manner, that the particles of dissimilar kinds of matter have different electrical states, and that the constituents of a compound retain their peculiar states while in composition, the rationale, Mr. Davy has offered of the phenomena of decomposition by galvanism, would yet be very far from being satisfactory.

If we take water, *instar omnium*, and consider it as a compound of oxigen and hidrogen, and these substances as having, in respect to one another, the negative and positive states: it will by no means follow, that oxigen must be negative, or hidrogen positive, to every other body. In like manner, although the two wires of a galvanic battery be, respectively, the one positive, the other negative, yet the negative wire will be positive to a body more negative than itself, and the positive wire will be negative to a body more positive than itself. Now as far as we know from experience, a repellent force is not excited between electrified bodies, unless they be in precisely the same electrical state. If therefore the electrical state of oxigen and of hidrogen remain stationary, there will be only one point of positive electricity, at which the positive wire will repel hidrogen, and only one point of negative electricity, at which the negative wire will repel oxigen: and at all other points of excitement, the positive wire will attract hidrogen, and the negative wire will attract oxigen. Hence, as water is decomposed by the action of the two wires, when from the circumstances under which they are made to act, and from their effects on our instruments, we know, that they are in different degrees of positive and negative electricity, it becomes impossible to consider the repulsions, Mr. Davy speaks of, as essential to the decomposition, such repulsions being very rarely, if ever, exerted: but the Water taken as an example.

the whole decomposition must be referred to the unequal attractions of the two wires; for each wire will attract both oxigen and hidrogen, but with unequal degrees of force; and these attractions will be modified and counteracted by the attractions of the opposite wire. If, for example, the positive wire attract oxigen with a force equal to 20, and hidrogen with a force equal to 10; and *vice versa*, if the negative wire attract hidrogen with a force equal to 20, and oxigen with a force equal to 10; the efficient attraction between the positive wire and oxigen would be equal to 10, and that between the negative wire and hidrogen would be equal to 10, and consequently the power, tending to separate the oxigen and hidrogen, would be equal to 20. If, therefore, we keep in mind, that the effect of the two wires increases with the difference in their electrical state; we must, as might be shown by numerical calculation, suppose, that hidrogen is more positive than the positive wire, and oxigen more negative than the negative wire. On this supposition, and on no other, it will appear, that, as the excitement of the two wires is augmented, their action on water should be more powerful: for the nearer the electrical state of the positive wire comes to that of hidrogen, and the electrical state of the negative wire to that of oxigen, the stronger should be the efficient attraction of the positive wire for oxigen, and of the negative wire for hidrogen. The same reasoning must apply to the decomposition of all bodies, and the constituents of every body, decomposed by galvanism, must be considered as having electrical states more widely different, than are those of the positive and negative wires of the galvanic battery. But this is shown to be impossible by Sir Isaac Newton's experiment with the pendulum, and by every kind of experiment with the electrometer.

Why is not decomposition effected by a single wire?

Admitting, for a moment, that the attractive and repulsive forces of the minute particles of matter, and the action of galvanic wires on compound bodies, are really such as Mr. Davy supposes, it would, I think, be difficult to explain, why decomposition is never produced by a single wire, however powerful may be the battery, with which it is connected; why decomposition is never effected, either by common or galvanic electricity, except when *two* conductors,

tors, in different electricial states, are made to act on each other.

Sect. III. It is an established fact, that from the contact and separation of dissimilar and insulated metals there is produced such a change in the electrical state of each metal, that, after the separation the one is found to be positive, the other negative in relation to surrounding bodies; but it appeared to me, (not having in mind the experiments of Wilke and Œpinus,) a matter of some doubt, whether the alteration in electrical state is the effect of contact, or of separation. To determine this point, in place of the small plate which usually remains on my electrometer, I adapted a copper plate about 5 inches in diameter. It is evident, that when this apparatus is placed on a common table, the copper plate will be connected with the wire and gold leaves, but will in every other respect be perfectly insulated; and, consequently, that, whenever a state, different from that of surrounding bodies, is produced in the copper plate, it will be indicated by a divergence of the gold leaves.

Experiment to show whether electricity be produced by contact or separation.

The apparatus, above described, being so circumstanced, that the tin foil of the electrometer was connected with the Earth, while the copper plate, the wire, and the gold leaves were insulated, I brought, by means of an insulating handle, a zinc plate, also about 5 inches in diameter, into contact with the copper plate on the electrometer; but although they remained some time in contact; there was no visible divergence of the gold leaves. On separating the metals, the gold leaves immediately diverged; on again bringing them into contact, if the charge of the zinc plate had not been removed, the leaves returned to their natural position: on again separating the plates the divergence took place as before, and similar phenomena appeared, as often as the experiment was repeated. If the charge of the zinc plate had been removed after the separation, the succeeding contact did not reduce the gold leaves to their natural state; but left a slight divergence in them; and when the plates were again separated they diverged in a greater degree, than after the preceding separation. Thus, by repeating the experiment, and discharging the zinc plate after each separation, the divergence was considerably increased; not however beyond

yond certain limits, which apparently varied according to the state of the atmosphere as to moisture: and it is worthy of remark, that the manner in which the plates are separated materially effects the result of the separation. If one be slid along the other, neither will evince signs of electricity. The contact and separation of two copper plates produced no sensible effect on the gold leaves. From these, and the experiments of Wilke and Œpinus, I feel myself warranted in concluding, that the electrical states of dissimilar metals, and other dissimilar bodies, are not rendered different by the contact of these bodies with one another, but by their separation after contact *. I would also, from analogy, extend my conclusion to the minute particles of dissimilar kinds of matter; and would say, that when in contact, as in composition, they possess their natural, or, as I have endeavoured to show, in similar electrical states; but that on their separation, as in decomposition, they acquire electrical states different from what they had while in contact, and consequently different from their natural electrical states; and that from such change in the electrical states of the constituents of a compound, in consequence of separation, analogous to what takes place in respect to the voltaic plates, the one set of particles becomes relatively to the Earth and surrounding bodies positive, the other set negative.

Electrical states of bodies rendered different by their separation. This applicable to minute particles.

The experiments, to which I have just alluded, appear to me perfectly sufficient to point out the fallacy of the explanation, which is very generally received, of the excitement of the galvanic pile; the whole of which rests on the assumption, that dissimilar metals, while in contact, are in different electrical states, the one being relatively positive, the other negative; which has been shown to be perfectly untenable. The following also I consider as additional and weighty objections to the hypothesis. 1. The voltaic plates only act when applied to each other by extensive surfaces. In the present most improved galvanic troughs, the metals are connected together by comparatively few points, and the contrivance has not only rendered the apparatus more convenient for use, but also more powerful. 2. Volta's plates act

Supposition that the metals in the galvanic pile are in different states, erroneous.

* An account of the experiments of Wilke and of Œpinus will be found in Dr. Priestley's History of Electricity.

only

only when the polish of their surface is preserved; the copper and zinc plates of a galvanic battery are always very much tarnished. 3. The galvanic apparatus can only be excited by a decomposable fluid, and this fluid is always decomposed, when the apparatus is excited. From these considerations, I am inclined to conclude, that the principles, on which decomposable fluids act in producing their peculiar effects on the galvanic battery, have not yet been accurately determined.

It will be a general, and I think perfectly correct statement of the facts, relative to the decomposition of bodies by galvanism, to say, that hidrogen, the alkalis, the metals, and certain metallic oxides are, immediately after their separation by galvanism from oxigen, and from acids, found at the negative wire; and that oxigen and acids, after their separation from the first class of substances, appear at the positive wire. I trust, however, that the experiments and reasonings, which I have adduced, are sufficient to prove, that the particles of dissimilar kinds of matter do not exist in different electrical states while in composition, but that they acquire a difference of electrical state in the act of decomposition: this difference of electrical state is, therefore, not the cause, but the effect of decomposition.

The difference or state is owing to decomposition having taken place.

It yet remains to be determined, on what principle the opposite wires of a galvanic battery act, when their action occasions the separation of the constituents of compound bodies. To do this, I by no means conceive it necessary to enter into an investigation of the remote cause of electrical phenomena; on the contrary, I think the question may be decided by a reference to well known and undoubted facts.

How is decomposition effected by galvanism?

If two conducting bodies, in different electrical states, be brought near to each other, the difference will be destroyed; and if the difference between the electrical states of the conducting bodies be considerable, while the operation is going on by which it is removed, the conducting bodies will frequently be fused. This happens not only to bodies easily fused, but also to very refractory substances; as the alkalis, the earths, the metals. The fact divested of all hypothesis is, that the action of differently electrified conductors occasions a repulsive force to be exerted

By a repulsive force analogous to that of caloric.

erted between the particles of the conductors, and is, in this respect, precisely analogous to that power, which in the language of modern chemistry is denominated caloric.

Mode in which this is effected in the case of caloric,

It has been long known, that caloric, aided by the affinity of a substance for one of the elements of a compound, is sufficient to effect the decomposition of the compound; and this fact is particularly observable in the reduction of metallic oxides, by heating them with inflammables, or metals. By these means the French chemists have lately succeeded in their attempts to decompose the fixed alkalis, and have obtained, in an uncombined state, their constituent elements, which appear to be oxigen, and a metallic base. The rationale of these decompositions is sufficiently obvious. The repulsive force of caloric separates the constituent particles of the compound; at the same time, by diminishing the cohesion of the inflammable, or uncombined metal, it renders its attraction for oxigen efficient; and hence the separation of oxigen from the oxide, and its combination with the uncombined metal, or with the inflammable. The oxigen, entering into a new combination, is removed from the sphere of chemical action, and thus its reunion with the metal, from which it had been separated, is prevented.

and in that of galvanism.

The decompositions by galvanism will, I think, admit of explanation on similar principles. The action of the two wires of the galvanic battery occasions such a repulsion, at a certain number of points, as separates the constituents of the compound, which is made a part of the circuit, and which must possess a degree of conducting power. The separation of the particles of dissimilar kinds of matter, which had been in contact, produces different electrical states in them: the one set of particles is, consequently, attracted with greatest force by the positive wire, the other set of particles is attracted with greatest force by the negative wire: the separated particles are thus placed beyond the sphere of chemical action, and their reunion does not take place.

Having, then, considered at some length the question proposed by Mr. Davy, I am satisfied, that we cannot admit the hypothesis which refers chemical phenomena to the electrical energies of the particles of matter. I am willing to allow, that it is highly ingenious, and that at first sight it has

really

really the appearance of a simple generalization of facts: but I think it has been shown, that the assumption on which it rests is contrary to experiment and analogy; that the assumption is incapable of explaining the phenomena on account of which it was taken up; and that these phenomena can be explained on principles unconnected with any hypothesis, and which are the result of experiment and observation.

III.

Observations on the Igniting, or Wire-melting Power of the Voltaic Battery, as proportioned to the number of Plates employed; with an Account of some Experiments on this Subject, made in conjunction with Mr. JOHN CUTHBERTSON; *by Mr.* GEORGE JOHN SINGER, *Lecturer on Chemistry and Natural Philosophy. Communicated by Mr.* SINGER.

IN a lecture recently delivered at the Royal Institution, Dr. Davy detailed some experiments of the French Philosophers, made with the intention of ascertaining the proportion, in which water is decomposed by different Voltaic combinations, the *number* of plates being subjected to variation. After some observations on the probable source of inaccuracy in these experiments, he proceeded nearly as follows: "There is still another very interesting subject of "inquiry, *which has not yet been touched on*; I mean the "proportion the igniting power of the battery bears to the "number of plates employed." The Dr. then proceeded to exhibit some experiments on this subject; they were made with a new apparatus fitted up in troughs of Wedgwood ware; the size of the plates 11 inches, by $4\frac{1}{2}$ inches. The result of these experiments was very equivocal, *two* batteries ignited *four* times the length of wire ignited by *one* battery; but *six* batteries ignited little more than *twice* the length that *three* could ignite. Dr. Davy supposed, that the rate of ignition might vary in higher numbers, obeying a different law to that which obtains when a few plates

Inquiry concerning the ratio of the power of igniting wires to the number of plates by Mr. Davy.

Anomalies in the experiments.

plates only are employed. Every practical electrician would, however, I am convinced, refer the anomalous results of these experiments to some inaccuracy in the apparatus, or to a difference in the density of the fluid with which the batteries were charged; as the irregularities obtained were by far too considerable, to have been produced by a difference so trivial as that existing between the number of plates employed on this occasion. The authority of a philosopher, so highly and so justly celebrated as Dr. Davy, may give extensive and respectable circulation to even palpable errours; it is therefore the imperative duty of every genuine friend of science, to examine assertions flowing from such a source, and to give to the public any *facts* he may be acquainted with, that militate against, or contradict them.

Experiments on the subject made some years ago.

As early as the year 1804, direct experiments were made to ascertain the quantity of wire ignited by different numbers of plates. Of this I presume Dr. Davy was not aware, when he stated, that "the inquiry had not yet been touched on;" he may not have read the 18th volume of the Philosophical Magazine, or the 7th and 8th volume of Mr. Nicholson's Journal, or Cuthbertson's Practical Electricity, where an account of these experiments is published. In the 7th volume of this Journal, page 207, a series of experiments with large batteries of plates of 4 inches, and of 8 inches square, is detailed by Dr. Wilkinson: the results of these experiments prove, *that the power of ignition increases in direct proportion to the number of plates employed*; and this law of increase is uniform, whatever be the size of the plates. If a battery of any given size melt any determinate length of wire, *two* such batteries will melt *twice* the length, *three* such batteries will *treble* the effect, and by *four* it will be *quadrupled*, provided the acid with which they are charged is of equal strength.

The power of ignition in the direct ratio of the plates.

Other experiments gave similar results,

In the 8th volume of this Journal, pages 97 and 205, a very accurate series of experiments is given by Mr. Cuthbertson. By a variety of trials he proves, that double the quantity of plates burns twice the length of wire; and he points this out as a distinction between the action of common and Voltaic electricity, but concludes, that the difference arises from the imperfection of the Voltaic apparatus;

tus; as on one occasion he obtained a different result by employing very large plates arranged as a pile. In subsequent volumes of this Journal are several other papers on the same subject; but enough has been quoted to show, that the inquiry has not any claim to originality. The conclusions of the first experimenters are however at variance with those of Dr. Davy. Dr. Wilkinson and Mr. Cuthbertson suppose, that the igniting power of a battery composed of plates of any given size increases in direct proportion to the number of plates. Dr. Davy infers from his experiments, that, when a few plates are increased, the increase is as the square of the numbers; but in combinations of greater extent the effect does not increase so rapidly. The apparatus employed by Dr. Davy differs in structure from that of the earlier experimenters; and as this might occasion some slight difference in the results, I did not consider it justifiable to decide on the accuracy of either, without new trials. With the assistance of Mr. Cuthbertson the following experiments were made; their results furnish some useful practical information in addition to the ascertainment of the object, for which they were expressly instituted.

with one exception.

As the apparatus was not the same,

the experiments were repeated.

The acid mixture employed to charge the batteries was of the same strength in all the experiments, (being previously mixed in a large vessel for this purpose). It consisted of 10 gallons of water, 5 lbs. of strong nitrous acid, and half a lb. of muriatic acid. A mixture of this kind being the most effectual wire-melting charge. Ten batteries, each containing 10 pairs of four inch plates, fitted up in troughs of Wedgwood ware; and one battery, of 50 pairs of plates of the same size, fitted up in a wooden trough, with glass partitions, constituted the apparatus employed. The plates in the troughs of Wedgwood ware were new, but the glass partitioned battery had been frequently employed before.

The acid employed.

The apparatus.

Two of the Wedgwood batteries rendered nine inches of iron wire, $\frac{1}{120}$ of an inch diameter, faintly red hot, when the contact was first made. This effect continued but a very short time. When it had wholly ceased, an interval of one minute was suffered to elapse, and at the end of this time

Exp. 1.

time the contact was again made. Three inches of the same wire were now rendered red hot with the same appearance as the nine inches in the first experiment.

Exp. 2. Four Wedgwood batteries were next employed. At the first contact 18 inches of the same wire became slightly red hot; and the contact was preserved, till the effect of ignition entirely terminated. One minute was suffered to elapse, when, on removing the contact, 6 inches of wire were ignited in the same degree as in the preceding experiments.

Exp. 3. An interval of three minutes was suffered to pass without contact; at the end of this time, two batteries rendered six inches of the same wire red hot, and four batteries produced a similar effect on 12 inches.

Remarks. The uniform result of these experiments, in which the igniting power increases in the same proportion, however variable the action of the battery, renders it highly probable, that in Dr. Davy's experiments the batteries were accidentally charged with acid mixtures of variable strength, the increase in his first experiment being as the square of the numbers.

Exp. 4. To ascertain whether the ratio of increase continued the same when a larger combination is employed, ten batteries were charged with fresh acid, of the same strength. Five of these ignited at the first contact 18 inches of the same wire as that employed in the former experiments; and on repeating the experiment with ten batteries, an effect precisely similar was produced on 36 inches.

Exp. 5. A short interval was suffered to elapse, when five batteries ignited 15 inches of wire; and the same effect was produced on 30 inches by ten batteries.

Exp. 6. Platina wire, $\frac{1}{130}$th of an inch diameter, was taken. Ten batteries (in a diminished state of action) maintained a white heat in 5 inches of this wire. On repeating the experiment with five batteries a similar effect was produced on $2\frac{1}{2}$ inches of the same wire.

The power in direct proportion to the number. These experiments indicate, that the conclusions of Dr. Wilkinson and Mr. Cuthbertson are legitimate; and they prove also, that the *igniting power* not only *increases* in *exact proportion* to the *number* of plates; but that this ratio of increase

increase is *uniform*, however variable the action of the batteries may be.

The troughs of Wedgwood ware have the partitions, which form their cells, at a greater distance from each other, than that of the glass partitions in the wooden trough; they of course require more acid to excite a given quantity of plates; and it has been said, that this circumstance promotes the continuance of their action. The results of my experiments speak a different language. The continuance of the action is influenced much more by the nature and strength of the acid mixture; and I have not observed, that in any case the separation of the partitions to a greater distance than $\frac{3}{8}$ths of an inch is attended with any advantage in this respect.

The continuance of the action depends more on the acid than the size of the cells.

At the commencement of the preceding experiments, a glass partitioned battery, of 50 pairs of four-inch plates, was filled with the same acid mixture as that employed in the troughs of Wedgwood ware. Its action was greatly inferior, in consequence of the oxidated state of the plates from former operations; but the continuance of its action appeared precisely similar, and at the conclusion of the experiments the effects were so nearly alike, as to admit of no perceptible distinction. At the first contact 9 inches of wire were ignited, and by allowing an interval of five minutes a similar effect was produced by a second contact; a circumstance which proves, that the voltaic battery requires, like the electrical machine, time to produce its full effect. This fact, as indicated by the sensation produced on the animal organs by a series of 600 small plates, was noticed many years ago by Dr. Wilkinson.

Glass partitioned battery.

Batteries require time to produce their full effect.

The preceding are part only of a series of inquiries on this subject, which have long occupied my attention, and which I purpose to detail in future numbers of the Journal; anxious only, that in experimental science assertions be supported by accurate experiments; and that, in the progress of philosophical discovery, the merit of the first labourers be not forgotten amidst the achievements of their successors.

No. 3, Princes Street, Cavendish Square,
April the 13*th*, 1811.

IV.

On the different Forces with which Tubes, Bars, and Cylinders, adhere to a Magnet. In a Letter from Mr. E. LYDIATT.

To Mr. NICHOLSON.

SIR,

THROUGH the medium of your scientific Journal I am anxious to obtain information on some magnetic phenomena, which I have lately noticed to have taken place on applying different shaped conductors to connect the poles of a horseshoe magnet.

An iron tube connecting the poles of a horseshoe magnet adhered with great force.

In preparing the introductory course of lectures on the philosophy of the mechanic arts, which I have delivered this season at the Scientific Institution, I had occasion to make a few experiments on the magnetic property of iron and steel; in the course of which I happened to place a piece of iron tube in contact with the two poles of a horseshoe magnet composed of thin bars; and found to my surprise, when I attempted to remove it, that a considerably greater force was required, than that necessary to separate the conductor which belongs to the magnet, which, as usual, was a square piece of iron with a ring attached, and presenting a flat surface equal to the combined polar surfaces of the three bars composing the magnet. This striking singularity induced me to ascertain the relative force required, to overcome the different degree of attraction.

Its adhesion to that of a bar as 23 to 10.

I first applied the conductor belonging to the magnet, and, by suspending weights from the ring, found, that it separated with 5lb. I then supplied its place with the tube, which was a piece of gun-barrel about two inches in length, attached longitudinally from one pole to the other; and by passing a wire through it, and twisting the two extremities into a hook, I suspended the weights, and found that 11½lb were requisite to separate it from the magnet. From this experiment it will be evident, that the relative degree of attractive force, exerted by the magnet on these two different conductors, is as 10 to 23. I repeated the experiment several times, and the results were invariably the same.

The

The line of contact of the tube, when the weights were first suspended, traversed the poles of the centre bar of the magnet only; but while they remained attached, I turned the tube till it stood in a diagonal direction with the extreme angles of the outside bars; but no difference of attraction was indicated, as it would not sustain more, or separate with less weight, than in its first position.

Change of position produced no alteration in the effect.

I then increased the width of the line of contact in the tube, with a file, to about 1/8 of an inch, and found that it separated with nearly a pound less weight: I increased its width still more, and the attraction was proportionably less.

On increasing the contact by filing the surface, the attraction diminished:

This led me to suppose, that the extraordinary degree of attractive force, by which the tube was held to the magnet in the first instance, depended entirely on the minuteness of the line of contact; and of course, that a solid piece of sound iron of the same diameter, would be similarly attracted. To prove this however, I procured a solid cylinder of iron the same length and diameter as the tube; but upon applying the weights, I was surprised to find it separate with less than half what was necessary to displace the conductor belonging to the magnet.

but a solid cylinder adhered more feebly than a flat bar.

These hitherto unexplained, and probably unobserved, phenomena, are submitted for explanation to such of your philosophic readers, as may have paid more attention to this subject, than I have had an opportunity of doing; in hopes of being gratified with some communications, which will not fail to be interesting, while they elicit a more extensive inquiry into that mysterious and neglected principle of nature, magnetism.

Yours, &c.

London, April the 10*th*, 1811. E. LYDIATT.

V.

An Answer to Mr. MURRAY'S *Observations on the Nature of Potassium and Sodium: by Mr.* JOHN DAVY.

To Mr. NICHOLSON,

SIR,

MR. Berthollet has estimated the proportion of water in common fused potash at 13·9 per cent; and Mr. Davy, from an

Quantity of water in potash.

an experiment on the action of silex on this hidrate, has concluded in his Bakerian lecture for 1809, that it contains, taking the potash formed by the combustion of potassium as a standard, about 16 or 17 per cent.

Mr. Davy's standard potash.

In the same lecture he has shown from the quantity of fused muriate, produced from a given weight of potassium in muriatic acid gas, that his standard potash has a much greater saturating power, than the hidrate of potash; that 100 of the former will neutralize the same quantity of acid as 120 of the latter.

Combined with boracic acid without evolution of water.

He has since ascertained, that, when potassium and powdered boracic acid glass are heated together in a tube of platina, both with and without red oxide of mercury, no water or inflammable gas is produced; and that the result is the same, when potash formed by the combustion of potassium is combined with boracic acid.

Common potash does not.

On the contrary, substituting the hidrate, or common fused potash, he has in one experiment actually collected about 15 per cent of water; and the loss of weight after the combination of the acid and alkali, in other similar experiments, indicated from 15 to 20 per cent.

Combustion of potassium in oximuriatic gas.

He has found too, that the only product of the combustion of potassium in oximuriatic gas is fused muriate of potash; that the same salt is formed; and oxigen gas evolved, without the least appearance of water, when potash from the combustion of potassium is used; and that water as well as oxigen is separated when hidrate of potash is employed.

Difference between the pure alkalis and their hidrates.

In addition to these circumstances, which are stated in Mr. Davy's last Bakerian lecture, a copy of which he has allowed me to peruse, there are physical properties also pointed out, distinguishing potash and soda from the hidrates; the former for instance require a much higher temperature for fusion than the latter, and possess greater hardness and apparently greater specific gravity.

Peroxides of potash and soda.

It is well known to those who have attended to the late progress of chemical discovery, that potash and soda are only to be procured by the rapid combustion of the alkaline metals, or by the after application of a red-heat; and that peroxides are formed when the combustion is feeble either in oxigen gas or common air. Messrs. Gay-Lussac and Thenard

nard first distinctly pointed out the nature of these peroxides, and described their properties. According to their statement, the peroxide of potassium contains three times the quantity of oxigen that exists in potash, and the peroxide of sodium half as much more as exists in soda*. These oxides have also been examined by Mr. Davy, and the general results of his experiments are conformable to those of the French chemists.

Messrs. Gay-Lussac and Thenard, using the same test as Mr. Davy had before applied to their hypothesis, making comparative trials of the saturating powers of the alkalis formed from the metals and of the common hidrates, were convinced, that potassium and sodium are not hidrurets; and consequently they adopted Mr. Davy's opinion, that they are simple bodies†. Trials of the saturating power of the alkalis.

Mr. Murray controverts this opinion in his paper, published in the last number of your Journal: Finding that potash from the combustion of potassium, has much the same saturating power as hidrate of potash, he infers, that the metals of the fixed alkalis are compounds of unknown bases and hidrogen. As this gentleman does not describe the manner in which he formed his potash; there is every reason to conclude it must have been by combustion in the atmosphere, in which case, it would have been principally peroxide; and an equal weight of it ought to have less saturating power than an equal weight of common potash. Since, therefore, Mr. Murray's hypothesis appears to be unfounded, since it is contradicted by the ample statement of clear and decisive facts already made, I shall conclude without examining the speculations connected with it.

I am, Sir, with great respect,

Your humble servant,

J. DAVY.

London, March 14th,
1811.

* Moniteur, July 5, 1810.

† At the end of this paper will be found a notice of these gentlemen's experiments; it is part of a Report of the Institute, published in the Moniteur already referred to.

Extract

Extract from the Moniteur of July the 5th, 1810, referred to in the preceding paper. Translated from the French by T. O. C.

Peroxides of the alkalis treated with acids.

"THESE oxides [the peroxides] present with some acid gasses phenomena worthy of attention. Messrs. Gay-Lussac and Thenard observed, that with carbonic acid gas the results were, an alkaline carbonate and an evolution of oxigen gas: that with sulphurous gas and oxide of potassium a sulphate and oxigen were obtained; and that with this gas and oxide of sodium the produce was only a great deal of sulphate and a little sulphuret: that not the slightest trace of moisture was given out in any case; and that the weight of the products obtained corresponded precisely to those of the oxide employed and the acid absorbed: Now as in the combustion of potassium and of sodium nothing is evolved, or no volatile product formed; we perceive, that, if these metals be hydrurets, it is a necessary consequence, that the sulphates and carbonates of potash and soda, and no doubt all the salts that have these alcalis for their base, contain as much water, as the hidrogen of these hidrurets can form by combining with oxigen, and that they retain it at a very high temperature; which is possible, but which nothing has hitherto proved. If it were so, a farther consequence would be, that potash and soda contain much more water, than Messrs. d'Arcet and Berthollet admit in them: for these alkalis would contain not only the water which is extricated on combining them with acids, but likewife that which the falt formed is capable of retaining. It was of fome use to determine directly the first of these two quantities of water; and this Messrs. Gay-Lussac and Thenard have done. For this purpose they converted into alkali, gradually and by means of humid air, feveral grammes of potassium and sodium, and saturated them with sulphuric acid diluted with water. On the other hand, having employed the same acid to saturate pure potash and soda that had been heated red hot; and having taken an account, in all the saturations, of the acid employed, as well as of the metal

Inferences.

Quantity of water in the alkalis examined.

metal or alkali employed also; it was easy for them to deduce the consequence they sought. Thus they found, that 100 parts of potash contain 20 of water, and that 100 of soda contain 24, supposing potassium and sodium to be simple substances. They have even verified this quantity of water with respect to soda, by treating over mercury in a curved jar a given quantity with a quantity, also given, of dry carbonic acid gas. The soda was placed on a small plate of platina, and gave out so much water the moment the temperature was raised, that this water trickled in abundance down the sides of the jar. We can even by these means, or by sulphurous acid gas, render the water sensible in 2 millig. [0·03 of a grain] of soda or of potash."

VI.

On the Nature of Oximuriatic Gas, in reply to Mr. MURRAY. *By Mr.* JOHN DAVY.

To Mr. NICHOLSON.

SIR,

MR. Murray, in his answer to the remarks which I ventured to make on his former paper, appears principally desirous of showing, that what my brother, Mr. Davy, has advanced as a theory respecting oximuriatic gas, is strictly an hypothesis. The concluesiveness therefore of Mr. Murray's answer depends on his success in proving Mr. Davy's views hypothetical; if he fails in this respect, he fails altogether, and the old hypothesis loses its asserted claims to attention.

Mr. Murray considers Mr. Davy's theory as hypothetical.

Mr. Murray first affirms, that Mr. Davy's theory is not a simple expression of facts, as I have represented it; that it is not a fact, that muriatic acid gas is a compound of oximuriatic gas and hidrogen, but an inference; and that the compositions of all the oximuriates are similar inferences. This I cannot admit. In the formation of muriatic acid gas, no substances, but those just mentioned, are concerned; the weight of the compound is the exact weight of

This not the fact.

of the two gases employed—nothing ponderable escapes; muriatic acid gas consequently is not inferred, but is immediately perceived to be, a compound of oximuriatic gas and hidrogen, and all other cases are analogous.

Mr. Murray's illustration from the combination of oxide of mercury and muriatic acid.

Mr. Murray, to convince me of the errour of which he conceived me guilty, respecting the nature of Mr. Davy's theory, has recourse to particular instances to illustrate his argument. He says: "I combine oxide of mercury and muriatic acid, and form calomel, I conclude therefore, that calomel is a compound of oxide of mercury and muriatic acid. I combine muriatic acid and potash, and by dissipation of the water I obtain a solid product, which I consider as a compound of the muriatic acid and potash, and I perceive in these conclusions no supposition, but a simple expression of facts." If Mr. Murray can combine oxide of mercury and muriatic acid, and form calomel, I have no objection to his conclusions; if the above is a simple expression of facts, the theory which expresses those facts must be correct. But I have not been able to witness such facts. I have found, that, when muriatic acid gas is admitted into an exhausted retort, containing red precipitate, corrosive sublimate, and not calomel, is formed; that water in plenty is simultaneously produced; and that much heat is generated, sufficient indeed, when the experiment is made on a pretty large scale, to revive some mercury by the expulsion of its oxigen. Mr. Murray, not attending to all the phenomena, has formed a false theory. Stahl, finding sulphur produced by heating charcoal with sulphuric acid, asserted, that sulphur is a compound of sulphuric acid and phlogiston; and Mr. Murray, knowing that different metallic compounds may be procured by treating different oxides with muriatic acid, asserts, that these compounds consist of muriatic acid and metallic oxides. In Stahl's famous experiment, carbonic acid gas, not being then discovered, escaped his notice; but the same cannot be said of water, which Mr. Murray has thus neglected. The preceding illustration of Mr. Murray at once demonstrates the real difference between Mr. Davy's theory and the old hypothesis; and that the former is, as I have represented it,

a simple

a simple expression of facts, and the latter a series of suppositions.

I shall studiously avoid discussion in the remaining part of this paper: it is my intention to confine myself to facts, which speak for themselves, and are the only legitimate supports of a theory.

Mr. Murray asserts, that Mr. Davy is obliged to *suppose*, that water is produced in the common mode of making oximuriatic gas from muriatic acid, by means of the black oxide of manganese. Mr. Davy has ascertained the *fact*, that oximuriatic gas and water are produced, when black oxide of manganese is heated in muriatic acid gas.

Water produced when oxide of manganese is heated in muriatic acid gas.

Mr. Murray imagines a great intricacy in some parts of Mr. Davy's theory, which does not really belong to it; for theory, being an expression of facts, must be as simple as the facts themselves. Mr. Murray, for instance, conceives, that, in the solution of muriate of potash in water, water is decomposed; and that it is recomposed at the moment of its expulsion by heat. These are conjectures. In Mr. Davy's theory, fused muriate of potash, I conceive, is a compound of oximuriatic acid and potassium; and the solution of muriate of potash is a compound of oximuriatic acid, potassium, oxigen, and hidrogen. The mutual decomposition of nitrate of mercury and common salt is another supposed complicated instance pointed out by Mr. Murray. It is this gentleman who imagines the changes complicated. The facts are merely these: sodium surrenders its oximuriatic acid to the mercury, and receives in return its oxigen and nitric acid, and thus calomel and nitrate of soda are very simply formed.

Mr. Davy's theory more simple, than Mr. Murray supposes.

Mr. Murray seems to consider every thing anomalous, that is not accounted for; thus the want of action between charcoal and oximuriatic gas is in his opinion an anomaly in Mr. Davy's theory. Can Mr. Murray account for the want of action between charcoal and nitrogen, and between the metals and nitrogen? and, if he cannot, does he consequently consider these facts anomalous?

Things not accounted for not always anomalous.

Mr. Murray doubts what I have alleged to be fact; viz. that the composition of muriatic acid gas is uniformly the same. I do not pretend to account for the results of Dr. Henry's

Mercury decomposes muriatic acid gas and forms calomel.

Henry's experiments; on which he rests his doubts. I have observed, that, when muriatic acid gas is left in a jar over mercury, the acid will slowly disappear, calomel will be formed, and at length nothing but hidrogen will remain.

Carburetted hidrogen and oximuriatic gas do not form carbonic acid.

I have stated in my first paper the general result, that carbonic acid is not formed, when dried carburetted hidrogen is detonated over recently boiled mercury with an excess of oximuriatic gas. Mr. Murray wishes to know how I ascertained this fact.—I considered the precipitation of charcoal, and no cloudiness being produced on passing the residual gas through lime-water, sufficient evidences of this.

Detonation of hidrogen, oximuriatic gas, and carbonic oxide.

Mr. Murray objects to the mode in which his experiment on the detonation of a mixture of hidrogen gas, oximuriatic gas, and carbonic oxide, was repeated, and is not satisfied with the results which are in opposition to his own. I have assisted my brother in again making this experiment; Mr. Hatchett and Mr. Brande were present. A mixture, consisting of 14·6 measures of oximuriatic gas, of 4 measures of hidrogen gas, and of 10 measures of gaseous oxide of carbon, was inflamed by an electric spark over recently boiled mercury; a condensation of half a measure only was produced by the explosion. Pure ammoniacal gas was added in excess, and, after the admission of water, there remained 13 measures of unabsorbable gas. Eight measures of oxigen being introduced, the mixture was inflamed; there was a diminution equal to 4 measures, and 8 measures of the residue were absorbed by a strong solution of potash *.

Now the 8 measures of carbonic acid gas formed indicate 8 measures of residual carbonic oxide; and, when the common air present is taken into account, with the difficulty

* The oximuriatic gas was procured from a mixture of common salt, black oxide of manganese, and diluted sulphuric acid; the 14 measures employed were found by a comparative trial to be contaminated by 2 measures of common air. The other two gasses had been previously dried by potash. 11·5 measures of the carbonic oxide, detonated with 16·5 of oxigen, were immediately diminished to 19; and by agitation with a strong solution of potash, there was a farther diminution produced equal to 11 measures.

of

of effecting the entire exclusion of moisture, no result more satisfactorily conclusive, that no carbonic acid was formed, could be expected: and we obtained a similar result in another experiment, in which we employed a strong solution of potash instead of ammoniacal gas, for absorbing the acid gas formed.

Compound of oximuriatic gas and oxigen.

I mentioned in a note to my former paper the discovery made by Mr. Davy of a gaseous compound of oximuriatic gas and oxigen. I stated the method of procuring it, and the property which it has of converting carbonic oxide into carbonic acid. Mr. Murray appears to think very lightly of this compound. But I can assure this gentleman, "notwithstanding it is procured (as he justly remarks) from the same materials as oximuriatic gas, and by a process apparently not much different from that which is usually employed," that Mr. Davy has found it to possess very different properties. Copper leaf, arsenic, and the common metals, for instance, which instantly inflame in oximuriatic gas, remain untarnished in this gas. And, what is extraordinary, it is *oxigen* in union which prevents the combustion of the metals from taking place: for when the combination is broken by nitrous gas, or a gentle heat, the oximuriatic gas, set free, acts as usual. The decomposition too of this gas by heat is so rapid, that it produces a loud explosion: and, if the quantity is large, a dangerous one: and it is a very singular circumstance, that it is attended with the evolution of heat, and even of light, notwithstanding there is a very considerable increase of volume. Mr. Murray may have remarked the difference of colour between common oximuriatic gas and the gas from oximuriate of potash; it is owing to an admixture of the newly discovered gas. When this gentleman learns, that the pure gas contains about half its volume of oxigen, he will probably no longer doubt, that it may be able to convert carbonic oxide into carbonic acid; and since *oxigen* united to oximuriatic gas deprives the latter of all those properties, which it was supposed to owe to loosely combined oxigen, he will probably adopt the new idea, that oximuriatic gas is a simple body. But if on the contrary he should still prefer the old hypothesis—the consequence is inevitable—

Its singular properties.

he

he must account for muriatic acid being a supporter of combustion when combined with a *single* proportion of oxigen, and a nonsupporter when combined with a double proportion, and for a variety of other anomalies, which it is needless to mention.

I am, Sir, with great respect,

Your humble servant,

J. DAVY.

London, March the 15th, 1811.

VII.

An Attempt to answer the Queries proposed by F. D. *in the Journal for April last: by* William Crane, *Esq. F. R. M. S. Edinburgh.*

To Mr. NICHOLSON.

SIR,

Questions on the production of hyperoximuriate of potash.

A Correspondent, in your Journal for April, has in a paper on the production of hyperoximuriate of potash &c. pointed out some errours, into which Mr. Davy has fallen, in accounting for the formation of muriate and hyperoximuriate of potash; also respecting the formation of muriate of ammonia and oxide of tin, on the addition of water and ammonia to the fuming liquor of Libavius.

He observes, that, "when the oximuriatic acid comes into contact with the oxide of potassium, we must suppose, that part of it from superior affinity displaces part of the oxigen, and combines with the potassium". He then proposes the following questions:—"How shall we in the first place account for this partial action? If a superior affinity exist between part of the oximuriatic acid and part of the potassium; how is it, that it does not subsist between the whole? How is it, that the whole oxigen of the potash is not set free, and the combination consist of muriate of potash only?" In answer to these questions; it may be observed, that there are many phenomena in chemistry, where a partial decomposition only takes place, as has been noticed and explained by Berthollet in his Chemical Statics.

Partial decompositions take place in chemistry.

His

His next questions are :—" But what becomes of that portion of oxigen which is liberated? Does it unite with the remainder of the oximuriatic acid, and so united, do they combine with the remaining oxide of potassium? or, is it attracted by the already saturated *oxide*, and that too in the face of a superior affinity?" According to the explanation which has been given by Mr. Davy, these objections certainly present themselves; but if we agree with Mr. Murray*, that potassium is the basis of the alkali united with hidrogen, a circumstance which I think that able chemist has proved from the experiments he has made, and from those of Gay-Lussac and Thenard, they are in a great measure removed. When hidrogen unites by combustion with oxigen, the product which is obtained is invariably water, which Mr. Davy supposes to be the union of these gasses in a neutralized state. Hence as the union of potassium with oxigen is always attended with combustion, there is great probability, that the hidrogen of the potassium unites with oxigen and forms water, and we obtain, instead of an oxide of potassium, as has been supposed, a hydrate; or pure alkali is the unknown base combined with water. That this is the case is also probable, from the very strong attraction alkali has for its water of crystallization, from which both Mr. Davy and Mr. Berthollet say it cannot be entirely freed at a very high temperature: after it has been freed from the water it holds in superabundance, I would suppose, it then requires the aid of a chemical agent, powerful enough to decompose the water it still retains, thus liberating the oxigen, whilst the hidrogen remains united to the unknown base, forming potassium. Again, as oximuriatic acid can unite with water, it requires no twisting of theory to suppose, that the hyperoximuriate of potash is a triple compound consisting of oximuriatic acid, water, and the unknown base, having, perhaps, by the combined affinity of the water and this base an excess of oximuriatic acid, and of course no evolution of gas would take place. This opinion might be extended a little farther, and we may account for the disengagement of oxigen from the hyper-

Farther questions.

Answer on the supposition that potassium is united with hidrogen.

* See Mr. Murray's paper, Number for April.

oximuriate

oximuriate of potash upon the application of heat, by the combined affinity of the unknown base and oximuriatic acid for hidrogen being enabled to overcome, by the aid of heat, the affinity of the oxigen for the hidrogen, which neither of them can effect separately.

Composition of the two muriates

His next observation is, that, as muriate of potash is a compound of muriatic acid and potash. "We must now suppose, that, when the oximuriatic acid first enters the solution of potash, part of it attracts from the water of the solution, a portion of hidrogen; and, being thus changed to muriatic acid, combines with the potash to form muriate of potash. The oxigen thus liberated unites to the other portion of the oximuriatic acid and the hyperoximuriate of potash is formed," which, he says, is a direct contradiction to the theory advanced to account for the liberation of oximuriatic acid in the retort.

owing to the two acids coming over.

To account for the formation of the muriate of potash, there can be no occasion to have recourse to the decomposition of the water; for, as muriatic acid is extremely volatile, and as the action of the oxide of manganese is not instantaneous; it is evident, that part of the muriatic acid will rise and pass over with the oximuriatic acid, particularly in the first stages of the process, and hence we find both the muriate and oximuriate of potash.

Decomposi-

Mr. Davy, in accounting for the production of water

[redacted]

metal for oxigen". Suppposing the composition of the water in the first instance to take place according to Mr. Davy's views, then, in the second, the oximuriatic acid is attracted from the tin by the ammonia, at the same time it attracts, in its turn, the hidrogen of the water; and as by the attraction of the ammonia the affinity between the oximuriatic acid and tin is weakened, the tin by this being enabled to attract the oxigen of the water, and the oximuriatic acid attracting the hidrogen, the water is decomposed, and the oxide of tin and muriate of ammonia are formed.

I am, Sir,

Your humble servant,

Edinburgh, April the 9th, W. CRANE.
1811.

VIII.

Experiments on Allanite, a new Mineral from Greenland, by THOMAS THOMSON, *M. D. F. R. S. E. Fellow of the Imperial Chirurgo-Medical Academy of Petersburgh.**

ABOUT three years ago, a Danish vessel† was brought into Leith as a prize. Among other articles, she contained a small collection of minerals, which were purchased by Thomas Allan, Esq., and Colonel Imrie, both members of this society. The country from which these minerals had been brought was not known for certain; but as the collection abounded in cryolite, it was conjectured, with very considerable probability, that they had been collected in Greenland.

Collection of minerals in a Danish prize.

Among the remarkable minerals in this collection there was one, which, from its correspondence with gadolinite, as described in the different mineralogical works, particularly attracted the attention of Mr. Allan. Confirmed in the idea of its being a variety of that mineral by the opinion of

One of these supposed to be gadolinite.

* From the Transactions of the Royal Society of Edinburgh.

† Der Fruhling, Captain Jacob Ketelsen, captured on her passage from Iceland to Copenhagen.

Count

Count Bournon, added to some experiments made by Dr. Wollaston, he was induced to give the description, which has since been published in a preceding part of the present volume.

About a year ago, Mr. Allan, who has greatly distinguished himself by his ardent zeal for the progress of mineralogy in all its branches, favoured me with some specimens of this curious mineral, and requested me to examine its composition; a request which I agreed to with pleasure, because I expected to obtain from it a quantity of *yttria*, an earth which I had been long anxious to examine, but had not been able to procure a sufficient quantity of the Swedish gadolinite for my purpose. The object of this paper is to communicate the result of my experiments to the Royal Society; experiments which cannot appear with such propriety any where as in their transactions, as they already contain a paper by Mr. Allan on the mineral in question.

Description of it.

Sect. 1. I am fortunately enabled to give a fuller and more accurate description of this mineral than that which formerly appeared, Mr. Allan having since that time discovered an additional quantity of it, among which he not only found fresher and better characterised fragments, but also some entire crystals. In its composition it approaches most nearly to cerite; but it differs from it so much in its external characters, that it must be considered as a distinct species. I have therefore taken the liberty to give it the name of Allanite, in honour of Mr. Allan, to whom we are in reality indebted for the discovery of its peculiar nature.

Allanite occurs massive and disseminated, in irregular masses, mixed with black mica and felspar; also crystallised; the varieties observed are,

1. A four-sided oblique prism, measuring 117° and 63°.

2. A six-sided prism, acuminated with pyramids of four sides, set on the two adjoining opposite planes. These last are so minute as to be incapable of measurement. But, as nearly as the eye can determine, the form resembles fig. 1, Pl. II; the prism of which has two right angles, and four measuring 135°.

3. A flat prism, with the acute angle of 63° replaced by one plane, and terminated by an acumination, having three principal

principal facettes set on the larger lateral planes, with which the centre one measures 125° and 55°. Of this specimen an engraving is given in the annexed plate, fig. 2.

Specific gravity, according to my experiments, 3·533. The specimen appears to be nearly, though not absolutely, pure. This substance, however, is so very much mixed with mica, that no reliance can be placed on any of the trials which have been made. Count Bournon, surprised at the low specific gravity noted by Mr. Allan, which was 3·480, broke down one of the specimens which had been sent him, in order to procure the substance in the purest state possible, and the result of four experiments was as follows.

4·001
3·797
3·654
3·119

In a subsequent experiment of Mr. Allan's, he found it 3·665. From these it appears, that the substance is not in a pure state. Its colour is so entirely the same with the mica, with which it is accompanied, that it is only by mechanical attrition that they can be separated.

Colour, brownish-black.

External lustre, dull; internal, shining and resinous, slightly inclining to metallic.

Fracture, small conchoidal.

Fragments, indeterminate, sharp-edged.

Opake.

Semihard in a high degree. Does not scratch quartz or felspar, but scratches hornblende and crown glass.

Brittle.

Easily frangible.

Powder, dark greenish-gray.

Before the blowpipe it froths, and melts imperfectly into a brown scoria.

Gelatinises in nitric acid. In a strong red heat it loses 3·98 *per cent* of its weight.

Sect. 2. My first experiments were made on the supposition, that the mineral was a variety of gadolinite, and were pretty much in the style of those previously made on that substance by Ekeberg, Klaproth, and Vauquelin. Experiments to ascertain its composition.

1. 100 grains of the mineral, previously reduced to a fine Silex.

powder in an agate mortar, were digested repeatedly on a sand bath in muriatic acid, till the liquid ceased to have any action on it. The undissolved residue was silica, mixed with some fragments of mica. When heated to redness, it weighed 33·4 grains.

Alumina.

2. The muriatic acid solution was evaporated almost to dryness, to get rid of the excess of acid, dissolved in a large quantity of water, mixed with a considerable excess of carbonate of ammonia, and boiled for a few minutes. By this treatment, the whole contents of the mineral were precipitated in the state of a yellowish powder, which was separated by the filter, and boiled, while still moist, in potash lie. A small portion of it only was dissolved. The potash lie was separated from the undissolved portion by the filtre, and mixed with a solution of sal ammoniac, by means of which a white powder precipitated from it. This white matter, being heated to redness, weighed 7·9 grains. It was digested in sulphuric acid, but 3·76 grains refused to dissolve. This portion possessed the properties of silica. The dissolved portion, being mixed with a few drops of sulphate of potash, shot into crystals of alum. It was therefore alumina, and amounted to 4·14 grains.

Metallic oxide.

3. The yellow matter, which refused to dissolve in the potash lie, was mixed with nitric acid. An effervescence took place, but the liquid remained muddy, till it was exposed to heat, when a clear reddish-brown solution was effected. This solution was evaporated to dryness, and kept for a few minutes in the temperature of about 400°, to peroxidize the iron, and render it insoluble. A sufficient quantity of water was then poured on it, and digested on it for half an hour, on the sand bath. The whole was then thrown upon a filter. The dark red matter, which remained on the filter, was drenched in oil, and heated to redness, in a covered crucible. It was then black, and attracted by the magnet; but had not exactly the appearance of oxide of iron. It weighed 42·4 grains.

Lime.

4. The liquid, which passed through the filter, had not the sweet taste which I expected, but a slightly bitter one, similar to a weak solution of nitrate of lime. Hence it was clear, that no yttria was present, as there ought to have

been,

been, had the mineral contained that earth. This liquid being mixed with carbonate of ammonia, a white powder precipitated, which, after being dried in a red heat, weighed 17 grains. It dissolved in acids with effervescence; the solution was precipitated white by oxalate of ammonia, but not by pure ammonia. When dissolved in sulphuric acid, and evaporated to dryness, a light matter remained, tasteless, and hardly soluble in water. These properties indicate carbonate of lime. Now, 17 grains of carbonate of lime are equivalent to about 9·23 grains of lime.

5. From the preceding analysis, supposing it accurate, it followed, that the mineral was composed of Deductions.

Silica,	37·16
Lime,	9·23
Alumina,	4·14
Oxide of iron,	42·40
Volatile matter,	3·98
	96·91
Loss,	3·09
	100·00

But the appearance of the supposed oxide of iron induced me to suspect, that it did not consist wholly of that metal. I thought it even conceivable, that the yttria, which the mineral contained, might have been rendered insoluble by the application of too much heat, and might have been concealed by the iron, with which it was mixed. A number of experiments, which it is needless to specify, soon convinced me, that, beside iron, there was likewise another substance present, which possessed properties different from any that I had been in the habit of examining. It possessed one property at least in common with yttria; its solution in acids had a sweet taste; but few of its other properties had any resemblance to those which the chemists, to whom we are indebted for our knowledge of yttria, have particularised. But as I had never myself made any experiments on yttria, I was rather at a loss what conclusion to draw. From this uncertainty I was relieved by Mr. Allan, who had the goodness to give me a small fragment of

The oxide examined.

 gadolinite,

gadolinite, which had been received directly from Mr. Ekeberg. From this I extracted about 10 grains of yttria; and upon comparing its properties with those of the substance in question, I found them quite different. Convinced by these experiments, that the mineral contained no yttria, but that one of its constituents was a substance with which I was still unacquainted, I had recourse to the following mode of analysis, in order to obtain this substance in a pure state.

Analysis.

Sect. III. 1. 100 grains of the mineral, previously reduced to a fine powder, were digested in hot nitric acid, till nothing more could be dissolved. The undissolved residue, which was silica, mixed with some scales of mica, weighed, after being heated to redness, 35·4 grains.

Silex.

Oxide of iron.

2. The nitric acid solution was transparent, and of a light brown colour. When strongly concentrated by evaporation, to get rid of the excess of acid, and set aside in an open capsule, it concreted into a whitish solid matter, consisting chiefly of soft crystals, nearly colourless, having only a slight tinge of yellow. These crystals, being left exposed to the air, became gradually moist, but did not speedily deliquesce. The whole was therefore dissolved in water, and the excess of acid, which was still present, carefully neutralised with ammonia. By this treatment the solution acquired a much deeper brown colour; but it still continued transparent. Succinate of ammonia was then dropped in with caution. A copious reddish-brown precipitate fell, which being washed, dried, and heated to redness in a covered crucible, weighed 25·4 grains. It possessed all the characters of black oxide of iron. For it was attracted by the magnet, completely soluble in muriatic acid, and the solution was not precipitated by oxalate of ammonia.

Another precipitate thrown down.

3. The liquid being still of a brown colour, I conceived it not to be completely free from iron. On this account, an additional quantity of succinate of ammonia was added. A new precipitate fell; but instead of the dark reddish-brown colour, which characterises succinate of iron, it had a beautiful flesh-red colour, which it retained after being dried in the open air. When heated to redness in a covered crucible,

crucible, it became black, and had some resemblance to gunpowder. It weighed 7·2 grains.

4. This substance attracted my peculiar attention, in consequence of its appearance. I found it to possess the following characters: This examined.

a. It was tasteless, and not in the least attracted by the magnet, except a few atoms, which were easily separated from the rest. Its characters.

b. It was insoluble in water, and not sensibly acted on when boiled in sulphuric, nitric, muriatic, or nitro-muriatic acid.

c. Before the blowpipe it melted with borax and microcosmic salt, and formed with both a colourless bead. With carbonate of soda it formed a dark-red opake bead.

d. When heated to redness with potash, and digested in water, snuff-coloured flocks remained undissolved, which gradually subsided to the bottom. The liquid being separated, and examined, was found to contain nothing but potash. When muriatic acid was poured upon the snuff-coloured flocks, a slight effervescence took place, and when heat was applied, the whole dissolved. The solution was transparent, and of a yellow colour, with a slight tint of green. When evaporated to dryness, to get rid of the excess of acid, a beautiful yellow matter gradually separated. Water boiled upon this matter dissolved the whole. The taste of the solution was astringent, with a slight metallic flavour, by no means unpleasant, and no sweetness was perceptible.

e. A portion of the black powder being exposed to a red heat for an hour, in an open crucible, became reddish-brown, and lost somewhat of its weight. In this altered state, it was soluble by means of heat, though with difficulty, both in nitric and sulphuric acids. The solutions had a reddish-brown colour, a slight metallic astringent taste, but no sweetness.

f. The solution of this matter in nitric and muriatic acid, when examined by reagents, exhibited the following phenomena: Action of reagents on its solution.

(1.) With prussiate of potash, it threw down a white precipitate in flocks. It soon subsided; readily dissolved in nitric acid; the solution was green.

(2.)

(2.) Prussiate of mercury. A light yellow precipitate, soluble in nitric acid.

(3.) Infusion of nut galls. No change.

(4.) Gallic acid. No change.

(5.) Oxalate of ammonia. No change.

(6.) Tartrate of potash. No change.

(7.) Phosphate of soda. No change.

(8.) Hydrosulphuret of ammonia. Copious black flocks. Liquor remains transparent.

(9.) Arseniate of potash. A white precipitate.

(10.) Potash. .... } Copious yellow-coloured
(11.) Carbonate of soda. } flocks; readily dissolved in
(12.) Carbonate of ammonia. } nitric acid.

(13.) Succinate of ammonia. A white precipitate.

(14.) Benzoate of potash. A white precipitate.

(15.) A plate of zinc, being put into the solution in muriatic acid, became black, and threw down a black powder, which was insoluble in sulphuric, nitric, muriatic, nitro-muriatic, acetic, and phosphoric acids, in every temperature, whether these acids were concentrated or diluted.

(16.) A plate of tin, put into the nitric solution, occasioned no change.

(17.) A portion being enclosed in a charcoal crucible, and exposed for an hour to the heat of a forge, was not reduced to a metallic button, nor could any trace of it be detected when the crucible was examined.

These properties approach those of cerium, These properties were all that the small quantity of the matter in my possession enabled me to ascertain. They unequivocally point out a metallic oxide. Upon comparing them with the properties of all the metallic oxides known, none will be found with which this matter exactly agrees. Cerium is the metal, the oxides of which approach the nearest. The colour is nearly the same, and both are precipitated white by prussiate of potash, succinate of ammonia, and benzoate of potash. But, in other respects, the two substances differ entirely. Oxide of cerium is precipitated white by oxalate of ammonia and tartrate of potash; our oxide is not precipitated at all: Oxide of cerium is precipitated white by hydrosulphuret of ammonia; while our oxide is precipitated black: Oxide of cerium is not precipitated

But with some difference.

tated by zinc, while our oxide is thrown down black. There are other differences between the two, but those which I have just mentioned are the most striking.

These properties induced me to consider the substance which I had obtained from the Greenland mineral as the oxide of a metal hitherto unknown; and I proposed to distinguish it by the name of *junonium*. Supposed a new metal.

In the experiments above detailed, I had expended almost all the oxide of *junonium* which I had in my possession, taking it for granted, that I could easily procure more of it from the Greenland mineral. But, soon after, I was informed by Mr. Wollaston, to whom I had sent a specimen of the mineral, that he had not been able to obtain any of my supposed junonium in his trials. This induced me to repeat the analysis no less than three times, and in neither case was I able to procure any more of the substance, which I described above. Thus, it has been out of my power, to verify the preceding details, and to put the existence of a new metal in the mineral beyond doubt. At the same time I may be allowed to say, that the above experiments were made with every possible attention on my part, and most of them were repeated, at least a dozen times. I have no doubt myself of their accuracy; but think that the existence of a new metal can hardly be admitted, without stronger proofs than the solitary analysis which I have performed. Junonium.

5. The liquid, thus freed from iron and junonium, was supersaturated with pure ammonia. A grayish white gelatinous matter precipitated. It was separated by the filter, and became gradually darker coloured when drying. This matter, after being exposed to a red heat, weighed about 38 grains. When boiled in potash lie, 4·1 grains were dissolved, of a substance which, separated in the usual way, exhibited the properties of alumina. Alumine.

6. The remaining 33·9 grains were again dissolved in muriatic acid, and precipitated by pure ammonia. The precipitate was separated by the filter, and allowed to dry spontaneously in the open air. It assumed an appearance very much resembling gum arabic, being semitransparent, and of a brown colour. When dried upon the sand-bath, it became very dark brown, broke with a vitreous fracture, and still retained a small degree of transparency. It was tasteless, An oxide,

tasteless, felt gritty between the teeth, and was easily reduced to powder. It effervesced in sulphuric, nitric, muriatic, and acetic acids, and a solution of it was effected in each by means of heat, though not without considerable difficulty. The solutions had an austere, and slightly sweetish taste. When examined by reagents, they exhibited the following properties:

examined by reagents,

(1.) Prussiate of potash. A white precipitate.
(2.) Oxalate of ammonia. A white precipitate.
(3.) Tartrate of potash. A white precipitate.
(4.) Hydrosulphuret of potash. A white precipitate.
(5.) Phosphate of soda. A white precipitate.
(6.) Arseniate of potash. A white precipitate.
(7.) Potash and its carbonate. A white precipitate.
(8.) Carbonate of ammonia. A white precipitate.
(9.) Ammonia. A white gelatinous precipitate.
(10.) A plate of zinc. No change.

appeared to differ in some respects from that of cerium;

These properties indicated oxide of cerium. I was therefore disposed to consider the substance which I had obtained as oxide of cerium. But on perusing the accounts of that substance, given by the celebrated chemists to whose labours we are indebted for our knowledge of it, there were several circumstances of ambiguity which occurred. My powder was dissolved in acids with much greater difficulty than appeared to be the case with oxide of cerium. The colour of my oxide, when obtained from oxalate, by exposing it to a red heat, was much lighter, and more inclined to yellow, than the oxide of cerium.

but this owing to the method in which it was procured.

In this uncertainty, Dr. Wollaston, to whom I communicated my difficulties, offered to send me down a specimen of the mineral called *cerite*, that I might extract from it real oxide of cerium, and compare my oxide with it. This offer I thankfully accepted *; and upon comparing the properties of my oxide with those of oxide of cerium, extracted from *cerite*, I was fully satisfied that they were identical. The more

* The specimen of cerite, which I analysed, was so much mixed with actonolite, that the statement of the results which I obtained cannot be

more difficult solubility of mine was owing to the method I had employed to procure it, and to the strong heat to which I had subjected it; whereas the oxide of cerium from cerite had been examined in the state of carbonate.

7. In the many experiments made upon this powder, and upon oxide of cerium from cerite, I repeated every thing that had been established by Berzelius and Hisinger, Klaproth and Vauquelin, and had an opportunity of observing many particulars, which they have not noticed. It may be worth while, therefore, without repeating the details of these chemists, to mention a few circumstances, which will be found useful in examining this hitherto scarce oxide. Some particulars respecting it not before noticed.

a. The precipitate occasioned by the oxalate of ammonia is at first in white flocks, not unlike that of muriate of silver, but it soon assumes a pulverulent form. It dissolves readily in nitric acid, without the assistance of heat. The same remark applies to the precipitate thrown down by the tartrate of potash. But tartrate of cerium is much more soluble in acids than the oxalate.

b. The solution of cerium in acetic acid is precipitated gray by infusion of nut-galls. Cerium is precipitated likewise by the same reagent from other acids, provided the solution contains no excess of acid. This fact was first observed by Dr. Wollaston, who communicated it to me last summer. I immediately repeated his experiments with success.

c. Cerium is not precipitated from its solutions in acids by a plate of zinc. In some cases, indeed, I have obtained a yellowish-red powder, which was thrown down very slowly. But it proved, on examination, to consist almost entirely of red oxide of iron, and of course only appeared when the solution of cerium was contaminated with iron.

be of much importance. The specific gravity of the specimen was 4·149, I found it composed as follows:

A white powder, left by muriatic acid, and presumed to be silica,	47·3
Red oxide of cerium	44·
Iron	4·
Volatile matter	3·
Loss	1·7
	100·0

d. The

d. The solutions of cerium in acids have an astringent taste, with a perceptible sweetness, which, however, is different from the sweetness, which some of the solutions of iron in acids possess.

e. The muriate and sulphate of cerium readily crystallise; but I could not succeed in obtaining crystals of nitrate of cerium.

Best method of obtaining the oxide.

f. The best way of obtaining pure oxide of cerium is, to precipitate the solution by oxalate of ammonia, wash the precipitate well, and expose it to a red heat. The powder obtained by this process is always red: but it varies very much in its shade, and in its beauty, according to circumstances. This powder always contains carbonic acid.

Essential characters of cerium,

g. I consider the following as the essential characters of cerium. The solution has a sweet astringent taste. It is precipitated white by prussiate of potash, oxalate of ammonia, tartrate of potash, carbonate of potash, carbonate of ammonia, succinate of ammonia, benzoate of potash, and hydrosulphuret of ammonia. The precipitates are redissolved by nitric or muriatic acids. Ammonia throws it down in gelatinous flocks. Zinc does not precipitate it at all.

h. The white oxide of cerium, mentioned by Hisinger and Berzelius, and described by Vauquelin, did not present itself to me in any of my experiments: unless the white flocks precipitated by ammonia from the original solution be considered as white oxide. They became brown on drying, and, when heated to redness, were certainly converted into red oxide.

As cerium, as well as iron, is precipitated by succinate of ammonia, the preceding method of separating the two from each other was not unexceptionable. Accordingly, in some subsequent analyses, I separated the cerium by means of oxalate of ammonia, before I precipitated the iron. I found, that the proportions obtained by the analysis above described were so near accuracy, that no material alteration is necessary.

Lime,

8. The liquid, thus freed from iron, alumina, and cerium, was mixed with carbonate of soda. It precipitated a quantity of carbonate of lime, which amounted, as before, to about

...ut 17 grains, indicating 9·2 grains of lime.

From the preceding analysis, which was repeated no less ...n three times, a different method being employed in ...h, the constituents of allanite are as follows:

Silica	35·4
Lime	9·2
Alumina	4·1
Oxide of iron	25·4
Oxide of cerium	33·9
Volatile matter	4·
	112·0

Component parts of allanite.

...mit the 7 grains of junonium, because I only detected it ...one specimen of allanite. The excess of weight in the ...eeding numbers is to be ascribed chiefly to the carbonic ...id combined with the oxide of cerium, from which it was ...completely freed by a red heat. I have reason to be... ...ve, too, that the proportion of iron is not quite so much as ...25 grains. For, in another analysis, I obtained only 18 ...ains, and in a third 20 grains. Some of the cerium was ...rhaps precipitated along with it in the preceding analysis, ...d thus its weight was apparently increased.

The iron probably overrated.

IX.

...servations on Three Papers of Mr. Davy. By Messrs. Gay-Lussac and Thenard*.

...N the Annales de Chimie for September last are transla...ons of three papers by Mr. Davy, sent to France by that ...ntleman, and entitled, 1, Observations on the Researches ...f Messrs. Gay-Lussac and Thenard relative to the Amal...am furnished by ammonia. 2. Examination of some ...bservations of Messrs. Gay-Lussac and Thenard on the ...acts respecting the Metals of the Alkalis. 3. Reply to

Mr. Davy's observations on the researches of Gay-Lussac and Thenard

* Abridged from the Annal. de Chim. vol. LXXV, p. 290.

Messrs.

answered. Messrs. Gay-Lussac and Thenard's Answer to the Analytical Researches, &c. These are followed by Observation on them by Messrs. Gay-Lussac and Thenard, some extracts from which will no doubt be acceptable to our readers; though a translation of the whole would take up much room to little purpose, as most of the facts have come before them in a different form. The following is its exordium.

Introduction. "The observations about to be read are divided into three parts. We shall merely relate our mode of viewing things, supporting it by reasons, which we believe to be preponderant. If by accident any expressions escape us liable to be misconstrued, we request our readers, and particularly Mr. Davy, not to do this. It is our intention unquestionably, to combat some of his opinions, because we do not always think with him: but while we combat them we wish to employ the language suited to truth, and merit the esteem of the celebrated chemist, whose talents have justly entitled him to that of all Europe, and more particularly ours."

Amalgam of ammonia not acted on by the air or sulphuric acid. On the first head these gentlemen say: "we have demonstrated, that the amalgam of ammonia has no action on the air, or on sulphuric acid; and it is totally impossible, that it should cover itself in the open air with a white powder of carbonate of ammonia." And again:

A compound of ammonia, hidrogen, and mercury. "In fine, we believe we have fully demonstrated, that the ammoniacal amalgam is nothing but a compound of mercury, ammonia, and hidrogen: for Mr. Davy opposes nothing to us, but that it is impossible to dry this amalgam thoroughly with blotting paper; and that the water, which covers it, combines with the ammonium, and reforms ammonia. But we know very well, that it is difficult to dry the surface of this amalgam with paper: and accordingly we take only the centre, after having cooled it to zero [32°] to increase its consistency; we introduce it into a very dry jar with very dry mercury; and immediately the amalgam decomposing gives out ammoniacal and hidrogen gas. Certainly this experiment is unobjectionable.

"However, as this experiment has not convinced Mr. Davy; and as perhaps he will tell us, that there is a little wate

water (which however cannot be) in the centre of this amalgam, we will relate another, to which we think he cannot reply. It is as follows.

"After having made a liquid amalgam of potassium, we poured it into a large cupel of moistened sal ammoniac, and obtained immediately, by the process for which we are indebted to Mr. Davy, a very bulky and very consistent compound of potassium and ammoniacal amalgam. Then, having removed with a knife all the upper part, we took out the interior parts with a very dry iron spoon, and immemediately put them into a tube almost full of mercury, which had been previously boiled. Afterward, having closed this tube, which was thus filled with mercury and the compound of ammoniacal amalgam with potassium, with a very dry stopple, we inverted it in mercury also well dried. The amalgam rose above the mercury, and was almost immediately decomposed, particularly by means of a slight agitation. But, in proportion as the decomposition went on, a pretty considerable quantity of gas was extricated; and this gas was always found to be a mixture of ammoniacal and hidrogen gas, in the proportion nearly of 2·5 to 1. Now will it be said, that the mercury or our vessels were humid? We can prove they were not; for, on pouring into them an amalgam of potassium, instead of a compound of potassium and ammoniacal amalgam, no gas was evolved. Or will it be said, that the interior of the ammoniacal amalgam with potassium contained a small quantity of water? But this is impossible, since water and potassium cannot exist together. Or, finally, will it be said, that we could not accurately remove with a knife the external portions of the compound of ammoniacal amalgam with potassium? But the experiment is so easily performed, that it can never fail.

Experiment to prove this.

"Thus the slightest objection cannot be made to this experiment, and it must be conclusive, even in the eyes of Mr. Davy. Besides, the result is easily understood: it is, the potassium, combining with a very large quantity of mercury, is so disseminated, that it can no longer act with sufficient force on the ammonia and hidrogen to unite them; so that the ammoniacal amalgam of potassium finds itself

The result explained.

itself in this case subjected to the same laws, as that which is formed solely of mercury, ammonia, and hidrogen and which cannot exist, except under the electric influence.

Lightness of the amalgam accounted for.

"If Mr. Davy admit, that the ammoniacal amalgam is a compound of mercury, ammonia, and hidrogen, he must admit also our explanation of the phenomena exhibited by its formation, or of the cause of its being five or six times as bulky as the mercury it contains. This explanation is perfectly natural. In fact, since the hidrogen and ammonia are scarcely more condensed in this amalgam, than they are in the state of gas, which is proved by the facility with which they escape from it, they cannot but considerably diminish the specific gravity of the mercury. The property that mercury has of being about 34000 times as heavy as hidrogen gas; and that which gold has of losing its lustre and ductility, and becoming soluble in all the acids by the addition of a few hundredths of oxigen gas, are facts as extraordinary."

Under the 2d head the French chemists observe:

Solid hydruret of potassium.

"Mr. Davy says, that he could never succeed in combining hidrogen gas with potassium, so as to form the solid hydruret of potassium; which we made known in 1808 No. 144 of the *Moniteur*, &c.; and on the preparation of which we gave some fresh information in No. 330 of the *Bibliothèque britannique*, in September, 1809. He imagines that in our experiments we paid no attention either to the solution of potassium in hidrogen gas; a solution, which according to him, occasioning probably a condensation of this gas, might have led us into an errour; or to the influence of the metal on glass; or to the circumstance, that from his observations, very small quantities of air or water give rise to a grayish powder, similar to what we announce as the hydruret of potassium. Our answer to all these observations shall be very simple. Let a certain quantity of potassium, as was said, *Bib. brit.* No. 330, p. 47, and of very dry and very pure hidrogen gas, be heated in a curved glass jar, thoroughly freed from air and water, and with its extremity immersed in water, the mercury will soon be seen to ascend rapidly in the jar, and at the expiration of a certain

Mr. Davy's remarks

answered.

tain time to be nearly stationary. At this period let the gaseous residuum be measured, and suppose it to be equal, for instance, to two thirds of the volume of hidrogen employed, it will be concluded, that one third of the hidrogen has been absorbed by the potassium. And, in fact, it may be expelled from it immediately, by heating the potassium sufficiently in the same jar in which the experiment has been made, and which is then full of mercury.

"Thus we find, that potassium absorbs a quantity of hidrogen, which is equivalent nearly to a fourth of what it gives out with water. We have repeated this experiment a great number of times, the result has always been the same. It is certain then, that a solid hydruret of potassium exists. The properties of this hydruret may be seen in the *Bib. brit.* as above quoted.*"

"Mr. Davy says, that potassium absorbs more ammoniacal gas dried by lime, than of common ammoniacal gas, in the proportion of 16 to 12·5. We have always observed on the contrary, that the absorption of these two gasses is perceptibly the same with an equal quantity of potassium, if the temperature be the same; as we have already shown in the *Bib. brit.* What Mr. Davy considers as potash is already, according to us, an ammoniuret. Potassium does not absorb more dry ammoniacal gas than moist.

"Mr. Davy says, that the ammoniuret, made with ammoniacal gas and potassium, does not give out, as we have advanced, the 0·6 of ammoniacal gas it contains; namely, 0·4 not decomposed, and 0·2 decomposed; or at least that these Ammoniuret of potassium gives out some ammonia undecomposed.

* "It is in the form of a gray powder, which has not a metallic appearance. It effervesces briskly with water, and gives out about one fourth more of hidrogen, than the metal it contains is capable of giving out. Placed in contact with mercury in the cold, it is gradually decomposed; an amalgam of this metal is formed, and all the hidrogen, to which it owed its pulverulent state, is evolved. If heated, its decomposition by mercury is almost instantaneous, and no more hidrogen gas is evolved than when cold. In fine, heated to an obscure red, it resumes the metallic appearance, and also evolves all the hidrogen, which the metal had absorbed." Ann. de Chim. vol. LXXII, p 266. Properties of the hidruret of potassium.

† Messrs. Gay-Lussac and Thenard say, in the place referred to, that a temperature somewhat elevated expels a great deal of ammonia from the olive coloured substance; and hence the quantity of ammonia absorbed by the metal is very variable, according to the temperature employed.

results

results are obtained only so far as there is moisture in the vessels employed. On this point we cannot accede to the opinion of Mr. Davy: neither our gasses, nor our mercury, nor our vessels, contain water; and yet we always obtain from this ammoniuret the 0·4 of ammoniac without being decomposed. This difference between our results and those of Mr. Davy does not depend on water, as he supposes, but on the high temperature, to which he exposes the ammoniuret."

unless overheated.

Under the 3d head Messrs. Gay-Lussac, and Thenard say:

Sulphuret and phosphuret of potassium treated with an acid.

"On treating the sulphurets and phosphurets of potassium with an acid, assisted by heat, as ought to be done, neither hidroguretted sulphur, nor hidroguretted phosphorus, is formed; and we always obtain even more phosphuretted hidrogen, than is requisite to represent the hidrogen of the potassium.

"Mr. Davy says, 1st, on treating the sulphuret of potassium with muriatic acid, he has obtained very variable quantities of sulphuretted hidrogen gas; and that in general less is evolved, than the potassium of this sulphuret would disengage of hidrogen from water: 2dly, that, on the contrary, on treating potassium with sulphuretted hidrogen gas, there is a greater quantity of hidrogen gas set free, than that which the potassium employed is capable of evolving in its contact with water.

"We have repeated more than fifty times our experiments on sulphur, sulphuretted hidrogen gas, and potassium: the sulphuret of potassium has always afforded us by acids a quantity of sulphuretted hidrogen gas, equal in volume to the hidrogen, that the potash was capable of evolving by its contact with water: and always too, on treating potassium with sulphuretted hidrogen gas, we have obtained as much hidrogen gas, as the potassium would have yielded with water.

"We affirm anew, that these results are certain.

Potassium heated with sulphur.

"Mr. Davy considers it as probable, that, on heating potassium with sulphur, a portion of potassium remains in the centre of the sulphuret of this metal. If but little sulphur be employed, this does not take place: still less then can it when a great deal is used, as is done by Mr. Davy.

"Mr.

"Mr. Davy says it is evident, that the method we employ in our endeavours to show, that his experiments on phosphorus and phosphuretted hidrogen are not accurate, do not apply to the case he has in contemplation. 'They have acted,' he adds, 'on the phosphuret of potassium with hot water, and thus they form phosphate of potash, and a large quantity of phosphuretted hidrogen gas; whereas, when strong muriatic acid is employed, the muriate of potassium is produced, and the oxigen is furnished solely, or principally to the potassium. We cannot form just conclusions, unless when the potassium alone is oxided; and my design in employing but a small quantity of acid was to oxide this substance alone."

Experiments on phosphorus and phosphuretted hidrogen.

"We shall observe, 1st, that we have treated the phosphuret of potassium not with hot water only, but with acids also; and that in every case we have proved, that more phosphuretted hidrogen gas was obtained, than was required to represent the hidrogen gas, that the potassium of this phosphuret was capable of furnishing with water; and that therefore Mr. Davy has nothing to object to the means we have employed to refute his opinion, or to demonstrate, that no oxigen exists either in phosphuretted hidrogen, or in phosphorus."

No oxigen exists in them.

"Mr. Davy accuses us of contradicting ourselves, as we have said, *Mém. d'Arc.*, vol. II, p. 304, that potassium, when heated in phosphuretted, sulphuretted, or arsenicated hidrogen, absorbs the phosphorus, sulphur, or arsenic, and a portion of hidrogen; and we say, *Jour. de Phys.* Dec. 1809, that potassium sets free all the hidrogen of phosphuretted or arsenicated hidrogen. In this there is nothing extraordinary. At first we employed an excess of potassium, and an absorption of hidrogen took place. But since, particularly when Mr. Davy had concluded from his experiments, that sulphur, phosphorus, and phosphuretted and sulphuretted hidrogen, contain oxigen, having examined anew the action of potassium on sulphuretted, phosphuretted, and arsenicated hidrogen gas; and for this having necessarily employed an excess of gas; we have seen, that, in this case, no portion of the hidrogen of the phosphuretted or arsenicated hidrogen is absorbed. Thus it appears, that we are perfectly consistent; since

Potassium in excess absorbs hidrogen from phosphuretted, sulph. or ars., hidrogen; not otherwise.

since we can at pleasure cause the hidrogen of these gases to be absorbed or not by the potassium.

Potassium absorbs phosph. hid. gas without flame,

"Mr. Davy observes, that we have said potassium absorbs phosphuretted hidrogen gas with flame; while on the contrary, as he has found, it absorbs it without flame. This is true, and the mistake has even occasioned us to make another, which Mr. Davy does not mention; it has led us to say, that potassium absorbs sulpharetted hidrogen gas without the emission of light. The fact is, these two experiments were made at the same time, and one was written down for the other. This may easily be conceived, for the phenomena are too visible not to be perceived. If we give this explanation however, it is not to exculpate ourselves from the mistake."

sulph. hid. with.

Arsenicated hidrogen contains oxigen.

"Mr. Davy complains of our having said, that, if he were acquainted with the action of arsenicated hidrogen gas on potassium, he would have inferred from it the existence of oxigen in this gas. We think the same still, because we do not obtain, on treating arsenic with water, a quantity of hidrogen gas representing that which potassium is capable of giving with water.

No oxigen in sulphur or phosphorus.

"Mr. Davy could have wished, that we had spoken of his experiments to demonstrate the existence of hidrogen in sulphur and phosphorus; and complains, that we have only endeavoured to point out errours. * * * * * But our only object was to inquire, whether these experiments demonstrated the existence of oxigen in these two substances: and, as no one of them proves this, and as the result of all are contrary to ours, we could not but draw inferences from them opposite to those of Mr. Davy."

Collection of Experiments.

"In a Collection of our Experiments, now in the press, we shall answer all Mr. Davy's objections, and endeavour to render him the completest justice."

X.

Observations respecting the Sensible Perspiration of the Dictamnus Albus, or Fraxinella. By Mr. Robert Lyall, *Surgeon, M. R. P. S. E. &c. Communicated by the Author.*

Assertion that the bastard dit.

It has been said, that in calm summer evenings the dictamnus albus evolves hidrogen gas, or a highly odorous inflammable

inflammable effluvium, which explodes when brought into contact with the flame of a candle; an opinion that is maintained in the latest botanical publications I have seen. tany emits an inflammable gas.

When I first became acquainted with the above notion, my curiosity was excited, and I longed for an opportunity to make the experiment, which was not very long denied me. The result of my observations I shall now relate in order, that the subject may be more accurately investigated. Experiment made on it.

I need scarcely premise, that the peduncles, the calyx, the outside of the corolla, and especially the tops of the filaments, and the germen of the dictamnus, are covered with glands of an oblong form, many of them supported on little pedicles, all of them of a beautiful red colour, and containing a somewhat viscid fluid. Glands on it containing a fluid.

On the 10th of July, about ten in the evening, the weather fine, and the temperature 66, I commenced my experiments on the dictamnus. By holding a lighted candle at the bottom of a raceme of flowers, inconsiderable explosions, or rather a hissing noise was occasioned, accompanied by light-blue coloured flame, which proceeded along the course of the peduncles, &c., and ascended even higher than the top of the stem; a good deal resembling an amusing experiment sometimes practised in the theatre, and often by boys, by means of powdered resin and a burning candle, &c. Immediately after the combustion, the surrounding atmosphere became tainted with odoriferous effluvia, exactly similar to what the healthy flowers, though much stronger, emit. July 13, I repeated this experiment, at the same hour as before. The evening was fine, but the plants were wet with the afternoon's rain. Scarcely any noise was produced; the experiment not succeeding as before. Exp. 1. Exp. 2.

At another time I brought home a raceme of flowers, and after it had stood with its end placed in water for two hours, I approached a burning candle to it, and little explosions followed. I replaced the raceme in the water; and next morning darkened my room, and made the same experiment, but heard no explosions. Since the 13th of July, I have frequently repeated the first experiment, but never have succeeded nearly so well as at first; a little hissing noise, attended with a small flame, only occurring now Other experiments.

 and

and then, occasioned in consequence of the bursting, I imagine, of the glands of new flowers; which, from their not being before developed, remained uninjured, during former experiments.

The glands destroyed in these experiments.

On examining the plants after the combustion, I observed, that the glands were completely destroyed; and thus I was led to suppose, that the resinous fluid which they contained was burnt during the explosion; and not that hidrogen, or any inflammable vapour was exhaled. Since after-experiments never succeeded so well as the first; and because the smell of hidrogen was never present, either before or after the experiment, I think I am strengthened in my opinion. At the same time, however, I confess, that I am not completely satisfied with my own observations, and therefore wish that some one, who has convenience, would not only repeat the experiments, but communicate the result of them to the public, and thus either ascertain the truth of what I have reported, or annul it altogether.

and no smell of hidrogen ever perceived.

XI.

Description and Use of a Rheumameter, to estimate and compare the Velocity of the Current of Rivers; by Mr. REGNIER, *Conservator of the central Museum of Artillery* *.

Different means employed to measure the velocity of rivers.

FROM Mariotte to the present day men of the first eminence have employed different means to estimate the velocity and force of rivers; and their methods, more or less ingenious, seem to leave nothing to be desired. I may incur the imputation of temerity therefore in bringing forward another, perhaps not equally good; but as it is very simple, attended with little expense, and requires no calculation, it may suit a great many persons, who are desirous of erecting mills or other works on rivers, with the velocity of which they are unacquainted.

Dynamometer applied to this purpose.

Mr. Gauthey, inspector general of bridges and highways, first employed my spring powderproof in the shape of a steelyard,

* Abridged from Sonnini's Biblioth. Physico-écon. March, 1810, p. 193.

steelyard, to ascertain the force of a stream on a given surface. His process is analogous to that of the bent lever balance, as described by Michelotti in his work on Experimental Hydraulics; but his method is not so simple, nor his apparatus so cheap and portable, as that of Mr. Gauthier.

Improvement in its application.

I have observed however, that the hand which holds the rod, to which the instrument is fixed, is liable unintentionally to give it an additional impulse. This inconvenience has led me to employ the steelyard in a different manner, which appears to me more convenient and more accurate, and affords the double advantage of measuring in distinct manners both the velocity of the current, and its absolute force on a given surface, so that the two modes of examination mutually check each other.

Apparatus described.

The apparatus consists of a cork log, or float, 10 cent. [4 inches] square, in the shape of a cube, so ballasted as just to sink to the level of the surface. A small reel, turning very freely, on which is wound a silk cord of a given length, to measure the distance the log should float. A small dynamometer, resembling that I constructed to measure the strength of threads of silk, cotton, or flax. With this apparatus, which may be carried in the pocket, the action of a current may easily be ascertained.

To the upper part of the log is fastened a silk cord, forming an acute angle, like the string of a kite; and to the point of the angle is hooked a red string two yards long, tied to a green string ten yards long, which is entirely rolled up on the reel. The other end of the green string is fastened to the reel, which the observer holds in his hand. A cord of two colours is used, to distinguish the part intended to measure the distance passed through from that which should be in the water with the log.

I have preferred a silk to a hempen cord, not only because silk is stronger and more pliable, but because it does not twist in the water, and retard the progress of the log. To satisfy myself of this, I have thrown into the water little pellets of paper, which floated freely by the side of the log, and the eye could perceive a sufficient uniformity in a distance of ten yards, the measure fixed on.

To

Method of using it.

To use it, a boat being anchored in the stream, the log is to be thrown into the water, and suffered to float away, till the whole of the green cord alone remains on the reel, which is stopped at this point by a catch. One person then looking at a seconds watch gives the signal, when the second hand begins its revolution, and instantly the other, who holds the reel, sets loose the catch; the log floats on, and the time it takes to run out the ten yards of line shows the velocity*.

To determine the absolute force of the current on the cube, slip the loop at the end of the cord off the knob on the reel, and hook it to the hole of the little dynamometer, and the number of degrees shown by the index will express the maximum of the action of the water on a surface of 16 square inches.

This action is not constantly the same, not only from the effect of the waves, but from the natural current, which appears not to be always regular. In fact we have observed in calm weather, without any apparent waves, that the force of impulse varied from one instant to another in the proportion of 6 to 8, or even more.

Experiments made with it

But the velocity has a great action, as will appear from a table of the experiments we made at Paris between the Pont des Arts and Pont-Royal, on the 20th of July, 1809. The weather was calm, and the Seine a little below its mean height, being at 1¼ met. [4 feet 11 in.] on the graduated scale of the Pont-Royal.

on the Seine.

First situation, 10 *yards from the side, opposite the wickets of the Louvre.*

Exp. 1. Veloc. in sec.,	25	Force in hectog. 2 to 3: in oz. avoird. 7 to 10½
2	26½	
3	26	

* The person who holds the reel in his right hand might dispense with an assistant, by holding in his left a stop watch, stopped at the end of the revolution of the seconds hand. He would only have to set loose the stop with the forefinger of the left hand, at the instant he disengaged the catch with the right, and stop the watch again the moment the line was run off the reel. C.

Second

Second situation, in the middle of the stream.

1	14½	} .... 6 to 9:	.... 21 to 31½
2	14		
3	14		

Third situation, 15 yards from the side, opposite the street des Saints-Pères.

1	28	} .... 1 to 2:	.... 3½ to 7
2	28		
3	28		

Though these data are not very ample, it is obvious, 1st, That the water at the sides of rivers has but little velocity: and

General conclusions.

2dly, That the velocity of the middle of the stream increases in an extraordinary degree the impulsive force; since the action produced on the log by a velocity of 10 met. [32f. 9 in.] in 14 seconds was from 21 ounces to 31½; while by a velocity of 28 seconds it was only from 3½ oz. to 7.

On comparing afterward our experiments with those of Mariotte, made about 1666 in the same place, we found a great deal of similarity in the results. By means of little balls of wax, ballasted so as to swim level with the surface, he estimated the velocity of the Seine, at its mean height, to be 150 feet in a minute, or 30 inches in a second. But when we made our experiments the Seine was only 4½ feet high, and at the time of Mariotte's it was 5 feet; a difference in height answering to the difference of velocity. And hence we may infer, that a century and half has made no change in the current of the river at this part.

The experiments of Mariotte in the 17th century.

The same experiments led us to compare the velocity of the Danube with that of the Seine. In the Journal de Paris, of the 11th of July, 1809, is a note from Baron Pakali, who says, that the velocity of the Danube, at its mean height at Ebersdorf, is 4½ feet in a second; so that we may consider it twice as rapid as the Seine at Paris.

Velocity of the Danube.

Explanation of the Plate.

Pl. II, fig. 3. *a*, a cube of cork, 4 inches square, bound round with packthread to strengthen it.

Explanation of the plate.

b,

b, a plate of lead, fastened to the bottom, to ballast the cube, so as to float level with the surface.

c c, knots from which proceeds a silk cord, forming an acute angle at the point *d*.

e, hook in the loop of the red cord about two yards long, tied to a green cord of ten yards, rolled up on the reel *f*, to measure the velocity.

g, a flat piece of hard wood forming a base to the reel, in the centre of which is a small rod of polished steel, on which, as an axis, the reel turns freely.

h, tail of the catch, on which the thumb rests, to let the reel move at the signal given.

Fig. 4. *i*, a small dynamometer, with an index, to mark on the arch the maximum of the impulse of the current.

Fig. 5. *k*, the log, floating in the stream.

l, the observer in a boat, holding in his hand the dynamometer, to estimate the force of the current, after having measured the velocity.

SCIENTIFIC NEWS.

French Institute.

French Institute.

AN analysis of the proceedings of the mathematical and physical class, during the year 1809, by Mr. Delambre, perp. sec., has just reached us.

Stability of the planetary system.

The question of the stability of the planetary system has been still farther pursued by Mr. Lagrange, who has examined it in a more general point of view, extending it to a system of bodies acting on each other in any manner whatever. He also purposes to investigate the relation of the planets round their centre of gravity, considering the deviation of their figure from a sphere, and the attraction the other planets exert on each of their particles.

Rotation of the Earth.

Mr. Poisson, as a continuation of his inquiry on the variations of the elements of the planets, has composed a paper on the rotation of the Earth. As Mr. Lagrange has noticed the extreme difficulty of this problem, we cannot be

be surprised to find, that formulæ have occurred to Mr. Poisson, the absolute summing up of which appeared to him impracticable. His object was to examine the influence of the term of the second order in the expression of the velocity of the Earth's rotation. These terms arise from expanding into a series the function expressing the sum of the products of the mass of each body attracting by that of the body attracted, divided by the mutual distance of these bodies. As it is impossible to calculate all these terms, the object is to bring forward only those that merit attention. Mr. P. accordingly examines in the first place, whether even those that depend on the Sun might not be neglected: and he finds, that they are always in fact very small.

As to the figure of the Earth, Mr. P. supposes, that, without the action of the Sun and Moon, the Earth would turn precisely round one of its principal axes. This is justified by the physical state of things, since we do not perceive in the altitudes of the pole, observed at different places, any of the oscillations, that would result from a different hypothesis, and the duration of which would be about one year. By similar considerations he expunges the terms relative to the other two principal axes, which can never become sensible but on hypotheses of little probability, which would give to the rotary motion of the Earth periods of less than two years, which have never been observed. He afterwards shows, that the equations to be summed up in the successive approximations preserve the same form; whence he concludes, that the axis of rotation will always coincide nearly with the shortest of the Earth's principal axes, and that the poles will always answer to the same points on the surface.

But, though the latitudes may not vary so as to deserve any attention, or to be perceptible to astronomers, is the rotary motion so uniform, as has been supposed? If its inequalities be of a very short period, and not very perceptible, they may escape our notice, and yet in a certain degree affect all our observations, and the consequences deduced from them. Suppose, for example, that the pole, instead of the 360° of its circle, passes only through 350°; and that the May there not be irregularities in it?

latitude

latitude of Paris observed at a given period should appear, in consequence of an oscillation then at its maximum, too great by 1″, the errour being proportional to the cosine of 0; the year following at the same time it would be proportional only to the cosine of 350°, and so on, till at the end of 9 years it would be nothing. At the end of 18 years however it would be 1″ in the opposite direction, whence a difference of 2″ might appear in the altitude of the pole; but so small an inequality in so long a period would not be noticed. To show the probability of this we might say, that Bradley, from a number of observations of the polestar in 1753, found the latitude of Greenwich 51° 28′ 41·5″, though from a still greater number he had before found it only 51° 28′ 38″. We may suppose therefore an oscillation of 2″ with a short period; or a greater oscillation, of which only a part has been observed. The latitude of the observatory at Paris too was found to be 48° 50′ 10″ at one time, and 48° 50′ 14″ at other times, by Lacaille, Cagnoli, Mechain, and myself. These differences might be ascribed to oscillations of at least 2″, and a period of about 15 years, so that there would have been 2½ periods between Lacaille and Cagnoli, and one only between Cagnoli and us. But I must add, that, having examined at large the observations of Bradley for five successive years, I have perceived no trace of these oscillations; that if there were one of 2″, it might frequently be confounded with the errours of observation; and that the difference of 3·5″ between the two results of Bradley might arise from his having changed his quadrant in the interval, and particularly from the errour of collimation, which for his old quadrant was 1·74″, and for the other 8″, not being known with sufficient precision, of which there are many instances. Thus we may take it for granted for the present with Mr. P. and astronomers in general, that there is no oscillation, or a very minute one; but of this we have no demonstration, and it is a point of sufficient importance, to be worth ascertaining with an instrument, in which no errour in the collimation is to be apprehended. For this it would be sufficient to observe for some years with Borda's circle the meridian altitudes of the polestar above and below the pole during the

Arguments for these.

But probably there are none.

though this remains to be proved.

the months of December and January: an oscillation, were it but of 2″, could then scarcely escape observation; as we are indebted to Mr. P.'s analytical investigation for the knowledge, that its period cannot be a complete year, so that the latitude must undergo a gradual variation, if observed regularly at the same period.

Mr. Poisson has also investigated some other formulæ, with a view to simplify them, and render them of more easy application. The first object, to which he has applied them, is the motion of a point attracted toward a fixed centre, according to any given function of the distance: and the second is the rotary motion of a body subjected to no accelerating force. His paper terminates with the following remarkable conclusion. "The perturbations of the rotary motion of solid bodies of whatever figure, to whatever attractive forces they are owing, depend on the same equations as the perturbations of the motion of a point attracted toward a fixed centre. Thus the precession of the equinoxes, and the nutation of the Earth's axis, will be expressed by the same formulæ, as give the variations of the elliptical elements of the planets.

Perturbations of revolving solids.

Mr. Legendre has given us some new theories in fluxions, and approximations of easy application.

Theorems in fluxions.

Messrs. Laplace and Bouvard have each investigated the problem of the motion of the Moon being such as always to present nearly the same face to the Earth. Mr. Bouvard shows, that there is no need of recurring to approximations. His method, though different from mine [Delambre's], is equally precise and direct; and his results agree perfectly with those of Mayer, thus affording an additional proof of the ability of that great astronomer.

Motion of the Moon.

Mr. Burckhardt has revised and enlarged a paper on the perturbations of the planets, which he composed in 1803, but had mislaid.

Perturbations of the planets.

To this is added another paper, which will conclude the volume for 1808, now about to be published. Theory has not yet been able, or has not ventured, to undertake the calculations necessary for determining the coefficients of the different inequalities of the moon, and they have been taken from observation. The method followed in these re-

Lunar tables.

searches

Lunar tables. searches is to leave in an indeterminate form, in the formula of the longitude or latitude of the Moon, all the unknown coefficients, multiplying them by the fraction which expresses the sine or cosine of the argument, on which the inequality depends. All the equations in which the same coefficient has the highest positive multipliers are brought together; another sum is made of those in which this coefficient has the highest negative cofactors; and from their comparison results the most probable value of the unknown coefficient, that which agrees the best with the observations. This method, which must have been followed by Mayer, has since by Masson and Buerg, and all who have calculated tables within these twenty years. This method is easy, and has no inconvenience but the length of the calculations when observations are taken by thousands; as must be done if we would determine the coefficients of those inequalities, which from their smallness have been neglected in the theory of the Moon: and Mr. B. now offers us a very simple method of abridging these calculations, since it dispenses with the calculating and summing up of all the sines of the argument.

Conceive a series of sines of arcs, forming a decreasing arithmetical progression from 90° to 90° minus a given limit y: Mr. B. has found, that we shall obtain with sufficient precision the value of the coefficient sought, by employing, instead of the mean arithmetical sine, the sine of y divided by the arc y. According to this idea he gives the rules to be followed in these researches, where we are liable to the vexation of finding after long calculations, that the inequality sought is null, or altogether imperceptible. As a trial of his method, Mr. B. has made a selection out of 1300 observations by Dr. Maskelyne, and proposed to determine an inequality, which should have for its argument the mean anomaly of the Moon, increased by the argument that regulates the inequality, the period of which is 180 years. Nine hundred observations gave him 4·7″ for the coefficient. He is desirous, that farther examination should be made of the goodness of an equation, which so well deserves to enter into the tables.

Mr. Burckhardt proposes some other calculations for the improvement

improvement of the lunar tables, which require only some one of sufficient courage to undertake the task.

Halley's comet.

In another paper the same astronomer has calculated the perturbations of Halley's comet, which reappeared in 1759, and is expected about 1835. He has found, that the attraction of the Earth will have altered the period of its revolution sixteen days.

Methods of determining azimuths.

Having formed the plan of a grand geodetic operation for joining observatories differing greatly in longitude, he was aware of the importance of an accurate determination of the azimuths to the success of his scheme, and in consequence examined the advantages and disadvantages attached to the different methods known.

Dip of the needle.

He has also determined the dip with two different needles, one of which gave 68° 47·1′, the other 68° 47·4′, on the 10th and 20th of August, 1809. Mr. Gay-Lussac had made similar observations with another compass about the same time; and as his dip differed some minutes from that of Mr. Burckhardt, these two gentlemen have agreed to repeat their trials, in order to ascertain, if possible, the cause of the difference.

Figure of the Earth.

Mr. Biot has read a note on the observations of the pendulum made at the two extremities of the meridian, namely at Formentera and Dunkirk, in company with Messrs. Arago and Mathieu, and on the oblateness of the Earth thence resulting. All these observations exhibit a surprising agreement with those made at Bourdeaux, Figeac, and Paris, by the same gentlemen and Borda; and give an oblateness differing very little from $\frac{1}{305}$, which I have deduced from a comparison of my arc with that of Peru.

Barometrical measurements.

Mr. de Prony having been of opinion, that Mr. Ramond's coefficient for barometrical measurements was too great for inconsiderable heights, and the original coefficient of Laplace better suited to them, Mr. Ramond has several times taken the height of various places near Clermond-Ferrand, by the barometer; and Mr. de Cournon measured the same heights trigonometrically. The heights were from 300 to 600 yards. The differences were from 1 yard to 0·05. Still the differences between the heights assigned to Mount Cenis by Mr. Ramond and Mr. de Prony remain

remain to be accounted for; since Mr. de Prony's barometrical measurement of that height is confirmed by the measurements of Mr. Daune, who had to take the levels during the construction of the road over it.

In order to introduce the use of the barometer in geodetical measurements, undertaken as preliminary operations in planning roads, and particularly for canals that have to traverse heights, which would be a considerable saving of time and expense, Mr. de Prony has undertaken a series of experiments at Paris and in its vicinity, to ascertain the coefficient best adapted to small heights. He verifies the barometrical heights by trigonometrical measurements with the repeating circle. Mr. Mathieu observes at the imperial observatory, and Mr. de Prony at the little observatory constructed for him over the pediment of the House of the Legislature. Mr. de Prony employs two micrometers, diametrically opposite, for adjusting the coincidence of the index with the tangent to the summit of the mercury, by means of which he makes this adjustment superior in accuracy to the measuring by the vernier.

Micrometers applied to the barometer.

Scarcely a private meeting passes without the class hearing some report on new machines or inventions, and on papers submitted to its examination by persons not yet members. As it is impossible to review all these, I shall only mention:

Propagation of light.

1. Researches on the velocity of light, by Mr. Arago, now member of the class, who has proved, that this velocity is the same, whether it come directly from the Sun or stars, or from a fire kindled on the Earth, or by reflection from the Earth, a planet, or any terrestrial body.

Fire-engine.

2. A fire-engine by Mr. Cagniard-Latour, who has made in it a very happy and inverse application of the screw of Archimedes.

Electrochemical inquiries.

In the physical department of the class the most prominent are the researches of Messrs. Gay-Lussac and Thenard in the brilliant career first opened by Mr. Davy; and though these gentlemen do not appear to contemplate every fact with the same eyes, the progress of science cannot fail to be promoted by the discussions that arise between them.

We

We are likewise indebted to Mr. Gay-Lussac for observations on the combinations of gaseous substances with each other, intended to show, that they always unite in simple ratios. Combinations of gases.

These observations are followed by a separate paper on nitrous vapour, and on nitrous gas as an agent in eudiometry. In this we see clearly the influence of quantities on the result of combinations. If two parts of nitrous gas and two of oxigen be mixed, nitric acid is produced, and one part of oxigen remains free. If on the contrary four parts of nitrous gas and one of oxigen be mixed, nitrous acid is produced, and one part of nitrous gas is left free. And, as nitrous gas is composed of equal parts of oxigen and nitrogen, we know the constitutions of the two acids with precision. Compounds of nitrogen.

Mr. Guyton de Morveau, in a series of experiments on the diamond, and substances that contain carbon, found that water was decomposed by the diamond at a very high temperature, and carbonic acid produced. Water decomposed by the diamond.

Mr. Sage has imparted his researches on the revivification of silver by mercury in the nitrate of silver; on an acetate of ammonia obtained from wood by distillation; on the analysis of the calcareous stone named typographical; on the magnesia contained in shells, madrepores, limestone, and arragonite; on an arenaceous iron ore; on an unknown petrifaction; and on a cupreous and ferruginous petrified wood. Mr. Sage.

(To be continued.)

To Correspondents.

A Constant Reader may find many of the articles he mentions in vols. XVIII, XIX, XX, XXII, and XXIII; others will be inserted as opportunities occur. His concluding suggestion will be considered.

METEOROLOGICAL

METEOROLOGICAL JOURNAL,

For APRIL, 1811.

Kept by ROBERT BANCKS, Mathematical Instrument Maker, in the STRAND, LONDON.

Day of MAR.	THERMOMETER. 9 A. M.	9 P. M.	Highest in the Day	Lowest in the Night.	BAROMETER, 9 A. M.	RAIN, noted at 9 A. M.	WEATHER. Day.	Night.
29	46°	46°	55°	39°	30.55		Fair	Fair
30	40	47	56	47	·37		Ditto *	Ditto *
31	47	45	48	43	·19		Cloudy	Cloudy
APR.								
1	46	47	54	40·5	·08		Fair	Fair
2	45	52	58	44	29·87		Ditto	Ditto
3	50	55	61·5	48	·96		Ditto	Ditto
4	52	50	60	43	30·05		Ditto	Cloudy
5	44	42	49	36	·12		Ditto	Fair
6	39	50	55	39	29·92		Ditto	Cloudy†
7	40	38	41	34	·48	·075	Snow	Ditto
8	36	39	42	32	·54		Ditto	Ditto
9	37	41	48	32	·64		Fair	Ditto
10	38	43	51	39	·83		Ditto	Rain
11	41	42	49·5	38	30·00	·025	Ditto	Fair
12	46	46	49	39	·19		Cloudy	Cloudy†
13	50	55	59	52	29·98	·220	Fair	Ditto
14	55	58	63	52	30·18		Ditto	Ditto†
15	55	55	59	52·5	·18	·120	Ditto	Fair
16	56	59	59	47	29·96		Rain	Cloudy‡
17	49	51	59	46	·85	·010	Fair	Ditto
18	51	50	58	47	·23	.160	Rain	Fair
19	50	53·5	59	48	·20	·055	Ditto	Cloudy
20	54	54	60·5	51	·30	·065	Fair	Ditto†
21	56	56	62	53	·42	·130	Ditto	Fair
22	56	58	65	55	·64		Showery	Cloudy
23	59	62	70·5	54	·60	·015	Fair	Fair§
24	58	60	68	53	·76		Ditto	Ditto§
25	57·5	56	62	52	·81		Ditto	Cloudy§
26	55	56	63·5	47	·76	·015	Ditto	Fair
27	56	57	65	44	·59		Ditto	Ditto

·890 Inch. since last Journ.

* Intervening Fogs. † Rain in the Night. ‡ Boisterous at 12 with rain. § Lightning, about 9 P. M.

[redacted]

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

JUNE, 1811.

ARTICLE I.

Description of a Method of Roofing Buildings securely with Flagstones. By RICHARD LOVELL EDGEWORTH, *Esq. F. R. S. M. R. I. A.*

To Mr. NICHOLSON.

SIR,

I Had occasion some time ago to roof a large building in an uncommon manner. I send you an account of it; as it has succeeded; and, as I believe, it may be useful in many places where slates and tiles are not to be had.

Gaol of Longford roofed with flags.

The gaol of Longford, in Ireland, which was built about twenty years ago, was covered with a circular arch of bricks; upon which broad flat stones, commonly called flags in this country, were laid with the best mortar that could be procured; these thin stones or flags were placed side by side; the lateral joints were filled up with mortar; and all the courses, as they descended, lapped over each other about two inches. After a short time the sun and frost cracked the mortar between the joints, and the rain found a way into every part of the building.

Penetrated by water.

Attempt to prevent it by a cement,

One of those *men of practice*, as they are called, from having been employed practically in building, undertook for forty or fifty pounds to remedy the evil, and by a *curious* cement to render the roof impervious to water. He laid on his hot cement of resin, and wax, and brick-dust, &c. The first summer shower passed off without penetrating through the joints, and the undertaker received his money; but in a short time things were as bad as ever, and the miserable creatures under confinement were drenched with rain and snow in every part of the prison.

did not succeed.

Expense of a lead or copper covering,

In the year 1809 the Grand Jury of the county of Longford desired, that I would endeavour to staunch this roof at any expense, that might be required. I received proposals for covering it with lead, and with copper: this could not be done for less than seven hundred pounds.

and of slating.

I then proposed to bolt down rafters upon the brick arch, so as to form a polygonous roof, upon which slates might be laid in the usual manner; but this I found would cost above four hundred pounds. It then occurred to me, that the flags on the roof might be so ordered, as to effect the intended purpose.

Management of the flags themselves to keep out the wet.

I took off a portion of the flags in fine weather, and without removing them from the top of the building I had them cut in the following manner; the flags (*a a*, Pl. III, fig. 3) were about three feet long, two feet or two feet six inches broad, and two inches and a half thick. The upper course was of fine even flags four feet broad, and each of considerable length, and under this course the roof was secure every where, except between the lateral joints. To prevent the rain or snow from penetrating between the upper and under courses or horizontal joints of the flags was the first object. For this purpose a groove was cut an inch deep in the surface of the upper part or top of those flags that were next the eaves; this groove was cut within one inch of the top of the flag. A similar groove was cut in the under side of the next course that lapped upon the lower course, and so on from the eave to the ridge; so that the upper flag or stone could hook upon the under one, as may be seen in the section, fig. 1. Pl. III.

The lateral

The next object was to secure the lateral joints. To effect

effect this purpose, grooves were cut into the upper surface along each side of every flag three quarters of an inch deep at one inch from the edge, see fig. 2 and 3, where a section or profile is given. To cover these lateral joints caps of lead were laid from the ridge to the eaves, a cap for each flag, or rather for every pair of flags. These caps, which had the appearance of a bead, were fastened over the rabbets or grooves of the flags by copper nails, *c*, driven through the caps into the juncture between the flags. These nails were made fast by slips of sheetlead *d d* fig. 3, put between the stones. A representation of the full size of the grooves in the stones of the lead cap and mushroom nails is given, fig. 3. Joints covered with lead.

Where holes were made through the lead caps, the water might find a passage; but this was prevented by preparing the holes in the lead in such a manner, as to stop the water above the hole, and to turn it aside from the direction which might be hurtful. The caps before they were laid in their proper places were turned upside down, and where nails were to pass, a burr or button, *b*, *b*, was punched in the lead half an inch deep, and by a proper tool passing through the punch, a hole was made in the centre of the button. The cap, when put into its place, covered the ridges of the flags between the grooves, so that no water could find an entrance between the joints of the flags; nor could any water rise above the tops of these buttons, because the descent of the roof would carry it off. Besides, the button or burr was covered by a *mushroom-headed nail*, the rim of which entered a little into the lead round the burr and prevented small particles of snow from gaining admittance. This effectual.

The holes in these caps might have been closed by solder; but whenever any work that is of difficult access is to be performed, it is always advantageous to have it executed by some one workman, in a manner that requires no difficult art or complicated apparatus. And I find that not one drop of water has penetrated through these joints during the two winters that have passed since they were covered according to this plan. Simplicity advantageous.

At the commencement of the business many difficulties with respect to the scaffolding were started. Very long ladders were requisite. Cripples hung on iron stauncheons Difficulties with respect to the scaffolding

 in

in the wall were deemed insecure; and the country workmen trembled at the idea of being perched so high from the ground without any apparent protection. I constructed eight light ladders, each six feet long; these were wider at one end than at the other, so as to permit them to be joined together by small bolts passing through the ends of both ladders. The ladders, thus joined, applied themselves commodiously to the circular roof, they were hung across the top and fastened by ropes, passing over the ridge of the roof to the iron bars of the windows of the upper cells on the opposite side of the gaol, which happened to be empty. On these ladders movable cripples were placed wherever a scaffold was wanting: on these cripples, which extended six feet from the roof, strong planks were laid, with ledges to prevent their slipping sideways; round this scaffold a coarse substantial handrail was tied. The passage to this scaffold was through a large opening in the top of the roof whence the workmen descended down the ladders to the lower platform, and thence to any part of the roof*.

surmounted.

Expense.

The scaffolding of this work cost but fifteen pounds, and the repair of the roof, exclusive of some other work that was carried on at the same time, came within one hundred and forty pounds.

As I may not have an opportunity of mentioning it in another place, I hope that you will excuse me for inserting a circumstance relative to this gaol, which is certainly not connected with the immediate subject of this letter; but as

* The moment the scaffold was finished, I went upon it myself, and from that time no objections were made. Notwithstanding all the precautions that had been taken, a fatal accident threatened the lives of the workmen. It has been said already, that the ropes which held the ladders were tied to the bars of the upper cells of the gaol. One morning, towards the close of the business, the principal workman found the ladders, and the scaffold that was attached to them, giving way. He had sufficient presence of mind to throw himself off the scaffold on the roof; as he was near the top, the slope of the roof was not sudden. He could therefore stick there till his companions relieved him.

The cause of this sudden failure it was impossible to foresee. A mad woman had been accidentally put for a single night into one of the upper cells; there by moonlight, with that mischievous alacrity which is often the accompaniment of insanity, she untied the cords, and left the scaffold without support.

it

it relates to public buildings of all sorts, it cannot be without some general interest.

In laying the first stone of the gaol of Longford twenty years ago, I placed in a cavity sunk in a large stone, under the S. W. corner of the building, several tiles, upon which, before they were baked, there were inscribed various memorandums for posterity, the Greek and Roman alphabets, the latitudes and longitudes of Paris and London, the variations of the needle, the nature and dates of various inventions, of gunpowder, of printing, of the steam-engine, of iron bridges, of the balloon; some of the discoveries of chemistry, and several remarkable events, with the names of celebrated books, and of their authors.

Documents placed under the foundation for posterity.

If this were done in various places in Europe, it might hereafter be not only gratifying to future curiosity; but might be useful to mankind. We have reason to believe, that fictile compositions are among the most durable substances that exist, and as we may, with the greatest ease, inscribe what we please on them before they are baked; it is but a small sacrifice to posterity, to give up an hour or two of leizure, from a hope, however feeble it may be, of preserving some of the discoveries, which have hitherto been made in art or science. Swift tells us, that a shrewd fellow inquired, why we did so much for posterity, when posterity has never done any thing for us. It is true, that posterity *has* never done any thing for us; but the idea of a posterity, that can bestow posthumous fame, has ever been and ever will be an excitement to present exertion. Our own immediate descendants reap the harvest which we sow, and nothing is more natural or more laudable than a wish to preserve our names among those who have been benefactors of society.

This recommended on other occasions.

Posterity.

Edgeworthtown, Ireland,
the 17*th of April,* 1811.

II.

Method of making any Ship's Boat a Life boat, to preserve the Lives of the Crew in imminent danger; by the Rev. James Bremner, *Minister of Walls and Flota, Orkney Islands**.

Case of Shipwreck.

HAVING a great many years ago witnessed a melancholy scene of shipwreck, and seen men perishing at little more than the distance of one hundred yards from the shore, it forcibly struck me, that though there was no possibility of getting from the shore to them, yet there was a great probability that means might be found, by which those in such situations might with safety be enabled to effect their escape to the shore; and farther considering, that the very precarious aid of some accidental piece of wreck (under every disadvantage and in a tempestuous sea) sometimes serves to save life, I was confirmed in the opinion, that some method might be devised, which, upon good grounds, would hold forth the promising prospect of safety in all the common and general cases of shipwreck. Hence it was, that to devise such a scheme became the object of my research ever after.

Plans for saving persons shipwrecked.

The following plans (especially the first) are so simple, and the effect so obvious, that I cannot allow myself to think that any seaman can entertain the smallest doubt, but that a boat so prepared would live in any sea whatever, could neither sink nor overset, and could carry in safety a number of people, in proportion to her size, over a bar, or from the wreck to the shore through any surf.

Buoyancy of empty casks.

That empty casks must float, almost wholly above the surface of the water, is so clear, that no person can be so absurd as to question it; and it is equally certain, that every cask will support weight of any kind in proportion to its size. In order then to accomplish the end proposed, there is only one thing more wanted, and that is, by means of sufficient seizings or holdings, to secure the casks in their places. Were you to tell a seaman, that he is not master of

* Trans. of the Soc. of Arts, vol. XXVIII, p. 135. The silver medal of the Society and twenty guineas were voted to the author.

this

this mighty operation, it is easier to conceive than to express the contempt he would feel, and the energetic reply he would probably make to such a supposition. If then these are undeniable points, it must follow, that wherever the boat can be had recourse to, all that is contended for in the plan must be granted.

It no doubt has been upon these simple and obvious principles, that those corporate and public bodies, and hundreds of seamen to whom the plan has been communicated, have so readily and entirely approved of it. But however respectable and authentic these testimonies (afterward to be mentioned) may be, I lay no stress upon that point, neither do I ask any credit for it, but freely submit my statements to the great body of seamen in general, leaving them to be judged of, not with liberality only, but with severity, considering that it would be a crime of the first magnitude, to advance a single argument or suggestion, that could have the smallest tendency to mislead, in a matter so solemn and important as where life and death are concerned.

Were I to go back to cases that are well known to have happened, I could easily point out many, wherein had this plan been thought of, there can be no doubt but it would have been attended with the happiest consequences; and probably the recollection of many seamen may furnish cases of the same kind, which have happened within their own knowledge.

I shall only add, that I expect no benefit or advantage whatever to myself from my perseverance and labours on this subject, nor reimbursement for an expense of some hundred pounds which it has cost me in repeated journies to Edinburgh and London, as well as in experiments, which a living of less than seventy pounds a-year could very ill afford; but I shall nevertheless reckon myself amply rewarded, if what I have to propose shall at any time, or in any case, prove the means of relieving from the deepest distress, and of rescuing from otherwise inevitable death, even a few of those who have had the misfortune to be involved in all the horrours of shipwreck.

Mariners are unavoidably exposed to incomparably greater Hardships and

hardships

of hard[illegible]s and sufferings, than are to be met with in any other line in human life.

While the labours of all others are moderate, and find relief at stated intervals by day, and repose by night, the seaman must contend with the storm so long as it lasts, and encounter danger at a moment's warning, whether at mid-day or midnight. Whilst the tempest rages, no respite can be allowed him; he must keep his station without intermission, and after toiling above strength and above measure, it is often his hard fate to be shipwrecked at last.

The complicated distress attending this frequent and fatal disaster it would be in vain to attempt to describe in any words; nor is it possible to conjecture nearly the number, which is added annually to the innumerable multitude of dead which the ocean contains.

Sometimes several hundreds in one ship are involved in this direful calamity, where the misery of each sufferer is increased, in proportion to the accumulated wo that surrounds him; the cry of despair is heard on every side, and in distraction each exclaims, What shall we do?

Amidst overwhelming waves and wreck, the mariner suffers in his person all that a living man can undergo, and in his mind all the anguish that despondence can create, heightened by the agonizing thought, that he is never more to behold wife, child, family, or friend; still however amidst all his sufferings an ardent love of life prevails, and the hapless mariner, struggling hard to preserve it, clings to whatever seems to promise a momentary reprieve.

In the mean time the wreck is rapidly giving way, some are washed away in one place, and others in another; those who remain redouble their efforts for life; but alas! they strive in vain; one decisive blow has dashed their last and only support to pieces, and all are going down together—a general shriek is heard—to be heard no more! the melancholy scene has closed, and neither survivor nor wreck is left behind.

Any plan then that has for its object to afford relief in situations of such extreme distress, and which seeks to extend the same benefit to thousands of perishing men in future

future ages, will no doubt meet with a favourable reception from every humane and benevolent mind.

But humanity and true benevolence are not merely speculative, but active principles; and wherever they really exist, the helping hand is instantly stretched forth, to execute the dictates of the feeling heart. True humanity an active principle.

As no subject can be more interesting to individuals than the present, or more important to society, may it not then be expected, that every friend to humanity and to his country will not only heartily wish success to the present plan, but also lend his best assistance to have it brought into all the practical effect, of which it may be found susceptible?

It is to be understood, that the plan is intended to apply to cases of shipwreck in general, and that it may very often succeed even in cases of extraordinary difficulty and peril.

This will comprehend the far greater number of all shipwrecks that happen, and the author thinks himself warranted to say, that no solid objection can be offered to the effectual operations of his plan to this extent, and that it will be found fitted to answer all the purposes of a life boat, by saving lives, where otherwise men must inevitably have perished.

At the same time he begs it may be understood, that he does not speak with this confidence from his own opinion only, however well-founded in principle and experiment it may be, but because the plan itself, after repeated investigation, has received the unanimous testimony and approbation of professional men, and of men too who must be allowed to be the most competent as well as the most respectable judges in the kingdom, namely, the Trinity House of Leith, in whose records a copy of it will be found. The plan approved by competent judges.

The Report of the Highland Society of Scotland confirms, that in their Committee appointed to witness the experiment at Leith there were naval men of that number who were competent judges, and in whose skill they could confide, and for this reference is made to the Appendix of their second volume.

It has been repeatedly submitted to the Trinity House of London. It was first submitted to them by Lord Melville, the treasurer of the navy, and their answer under the band of their

..eir secretary is inserted in the forementioned Appendix, ..igned James Court.

In the next place, the plan has been laid before the ..yal Humane Society, and they, not being naval men, do submit every essay of that nature to the Elder Brethren of the Trinity; and in consequence of their approbation a premium of five guineas was given by the R. H. S., as appears from their printed Reports 1800 and 1801.

And to these attestations might be added the subscribed approbation of more than one hundred ship masters, whom the author had occasion to see only accidentally, and whose subscribed names are now in his possession.

It is under the sanction of such authorities and documents, that it is now offered to the public, and they are such as must be satisfactory to every impartial and candid mind.

They have been obtained without interest, favour, or friend, and small premiums have been given without the author's knowledge, till informed by letter that his plan had received this mark of approbation.

It is impossible therefore to ascribe so honourable testimonies and gratuitous bounties to any other motive than to the conviction of the utility and efficacy of the plan, and an ardent desire to promote an object so devoutly to be wished, as the preservation of lives in cases of shipwreck.

The inventor trusts his statements will show, that he is not unacquainted with his subject: and he shall only add, that he has had more than forty years experience in the use of boats, among dangerous tideways and rapid currents, such as the Pentland Frith, and all the other channels among the Orkney Islands; and that he has been several times at sea on shipboard, in storms that were attended with shipwrecks; and that from such experience he is perfectly convinced, that his plan is sound and unexceptionable, and is confident that the period is not very distant, when it will come into as great repute and general use as lifeboats, properly so called, are now known to be.

The plan may be executed upon boats of all dimensions, and the largest, provided they could be got out, would be found the most advantageous: but, all circumstances considered, the size deemed in general best adapted for the purpose

purpose would be any boat from sixteen to twenty feet in length, which is to be prepared as follows.

Reference to the Plan of the Rev. Mr. Bremner's Preparation of Ship Boats as Lifeboats, Pl. III, *fig.* 4 *and* 5.

Two additional ring-bolts are to be fixed in the keel withinside of the boat. One to be placed one third of the boat's length from the stem. The other one third from the stern. Two auger bores are to be put through the keel withoutside, and close to the garboard stroke. One of these bores to be put about half way betwixt the ring in the stem, and that next to it in the keel. The other about half way betwixt the ring in the stern, and that next to it in the keel. Preparation of a ship's boat to be used as a lifeboat.

Plugs may in ordinary be put into these bores, to be struck out, when occasion requires.

Those ring-bolts which are in ordinary in every ship's-boat, the two additional ring-bolts in the keel, and the two augur bores, are all intended as secure points of fixture, to which seizing ropes are afterwards to be attached.

In the next place, two tight empty casks, (see fig. 4.) are to be provided, of such dimensions that their length may fit to the width of the boat, when laid athwart ship, and their diameters to be about three feet, and if larger so much the better. Casks.

Each cask must be furnished with a sling on each end, and each sling to have two eyes on it, about six inches asunder, and the slings so put on the cask as that the eyes may be on the upper side when laid into the boat, that the seizing rope may pass through those eyes, in their way from ring-bolt to ring-bolt.

One of these casks, so prepared, is to be laid in forward, and the other aft; and each cask so near its respective ring in the keel, as only to leave sufficient room for passing the seizing rope through the ring in the keel.

By this means the vacant space, to be then filled up with cork, will be left betwixt the cask and the bow forward, and betwixt the other cask and the stern aft.

The requisite quantity of cork, according to the dimensions of the boat, and the quality of the cork, may be about Cork.

a hundred

a hundred and a half, or two hundred weight, for each end of the boat, and that for each end ought to be made up into two separate bundles, each bundle being fitted to the width of the boat, and the uppermost one forming an arch from gunwale to gunwale.

The cork is to be made up in canvas, done over with soft pitch for preservation, and each bundle marked and numbered according to its place.

The casks and cork being laid into the boat, seizing ropes are then to be applied for securing them in their places.

Method of securing the casks and cork.

Here it is to be observed, that the single turn of rope which is to go through the augur bore in the keel and round all, should be the first made fast, that the other seizing rope (which we shall suppose to have been made fast to the ring in the stem) may, in passing through the eyes on the sling, take in the surrounding rope betwixt the two eyes, which will thereby prevent the surrounding rope from slipping to either side of the cask.

The seizing rope, having passed through the eyes on the sling, is then to be passed on through the ring in the keel, and thence back again in the same manner, through the eyes on the sling on the other end of the cask, to the ring in the bow; and lastly, the seizing rope is to be brought directly from the ring in the stem to the ring in the keel, by which it will cross the cask at the bung or middle part of it: the other cask and cork aft are to be secured in the same manner.

The preparation will be completed by attaching a bar of lead or pig-iron, of about two hundred weight, to the keel within side, by means of the ring-bolts in the keel or otherwise.

Variation in the plan.

The same plan may be executed with equal effect, and nearly with the same expedition, by the following alteration and arrangement.

Instead of one large cask, two less ones may be used in each end of the boat.

These are to be laid in lengthwise, fore and aft, in the boat, alongside of each other, and both together ought to fill the width of the boat.

These must also be furnished with slings on each end, and

and with two eyes on each sling, and these eyes so placed as to be about two inches above the horizontal diameter of the cask, one eye being on each side of the cask when the sling is put on.

The seizing-rope, being now made fast to the ring in the stem, is to be passed through the eyes on the slings on one side of the cask, then through the ring in the keel, and so back again through the eyes on the slings on the other side of the same cask, to the ring in the stem. The rope is then continued on till it has passed in the same manner on both sides of the adjoining cask, and the last turn is to be made directly from ringbolt to ringbolt, passing over and above the surrounding rope, which will thereby be brought down in the middle betwixt the two casks, and made closely to compress them on each side.

The same process is to be followed as to the casks aft, where the dimensions of the boat will admit of it, and where otherwise one large cask athwart ship may be used, as in the plate, fig. 5. It was in this manner that the experiment at Leith, hereafter to be detailed, was made, and all the cork that was used on that occasion was about one hundred weight put into the narrow part of the boat aft, in order to raise a common porter cask placed above it to a convenient height. The preparation of the cork bundles in this case will differ somewhat in their shape from those in the former plan, but as the purpose of them is the same, namely, to fill up the vacant spaces betwixt the cask and the boat, a particular description of them seems quite unnecessary; only it may be observed, that as the diameters of the casks forward are considerably less than that in the former plan, so much of the cork ought to be placed underneath, as may serve to raise the upper side of the casks about four inches above the gunwales, it being evident, that the higher they can be raised with sufficient security, the more effectually all possibility of overturning will be prevented.

The same quantity of ballast is to be used in this case as in the former, and is to be applied in the same manner.

With respect to boats of small vessels, a single cask forward and another aft, without any cork, will be sufficient. Boats of small vessels.

Each cask to be about the size of a hogshead, and to be set

set on end, or leaning obliquely towards the rings in the stem and stern, to which they are to be secured, and at the same time to two other rings placed in the keel, proper for that purpose: these casks, from their position and power, would effectually prevent sinking or upsetting: and as the crews of such vessels are few in number, their boats might support them safely through any breach into shallow water.

Advantages of timely preparation.

The foregoing plans are founded upon unquestionable principles, and constructed according to a regular method. They keep in view the difficulties to be encountered, and provide against them by making a few necessary preparations in due time. Were this attended to, all the confusion and embarrassment which arise from sudden alarm, and the distress that must attend a total want of suitable means, would be prevented, and an encouraging prospect of safety held out even in the most perilous situations.

The want of timely forecast, and the neglect of means that were in our power, never fail to occasion the bitterest self-reproach, and the most painful vexation, whenever we are overtaken by misfortunes, which a little prudence might have prevented.

A third plan suggested.

Having however but too much reason to apprehend, that such prudential provisions as have been stated will still be neglected, in spite of every suggestion and consideration that can be urged, I shall now propose a third plan. Though inferior to the former, as a ship with jury masts, torn sails, and a temporary rudder, is to one in perfect good condition; yet, considering that this inferior plan, like the disabled ship, may gain what was despaired of, and save what was given up for lost, I proceed to state it:

Casks alone.

This plan will consist in the application of casks only. These, if stowed closely and so as to fill up as well as possible one third part of the boat forward, and one third aft, would effectually prevent the boat from sinking or oversetting.

Upon this plan, in order the better to secure and combine the casks, the end of a sail should be in the first place thrown into the bottom of the boat, and the casks being stowed upon it, the other end of the sail should then be doubled over all: the seizings are then to be made through holes

holes struck any where through the bottom and sides, wherever the passing of a rope may be found necessary, or of any use for confining the casks.

Holes in the bottom of a boat advantageous.

The constant and general idea, that the utility of every boat depends upon the tightness of her bottom, and her completely resisting the admission of water, opposes itself strongly and almost irresistibly to the directly opposite idea, that water freely admitted could do no injury; nay, so strong is the received opinion, that it may be very difficult to persuade some, that large openings in the bottom would prove a real advantage; it is however undoubtedly true, that in the present plan this would really be the case.

It is therefore very material to observe, that neither the number nor the size of the holes struck through is of any consequence, as to the water in the boat; on the contrary, they would be so far from being detrimental, that, to a certain extent, they would be of advantage, as they would serve to discharge, in proportion to the buoyancy contained, whatever top-water might be withinside, above the level without, and which the boat would otherwise retain as a load and dead weight, if she were every where perfectly tight: whereas, in proportion as the buoyant power operated in raising her, the top-water would instantly subside through the holes in the bottom, and thereby render her more lively, and to swim higher out of the water.

From not attending sufficiently to the fact now stated, it has probably happened, that the plan we are at present describing has never been attempted; but whoever will take the trouble to consider the matter a little may soon be convinced, that they may, *without scruple or hesitation, make as many holes, and of whatever size,* as they may judge necessary for passing ropes, wherever they can serve for effectually securing the casks in their places.

The fastenings to be applied to the strongest parts.

The only point chiefly to be attended to is never to attach ropes to any tender part of the boat, such as the gunwales or thwarts, but to such parts as possess the greatest strength, and in which entire confidence may be placed.

As the largest boats have strong timbers, this plan might probably succeed best if applied to launches and long-boats.

Ballast.

Small anchors that have iron stocks, and which could be laid

laid in the bottom of the boat, would serve for ballast, though probably ballast in large boats would not be very necessary.

Holes. The holes to be struck through may be pierced with a marling-spike and mallet betwixt the timbers.

Buoyancy of casks. The power and effect of empty casks is well known, the application of them being a common expedient, used almost every day for the purpose of floating stranded or bilged vessels of great burden. How easy then it must be, by the same means, to render a boat buoyant to any degree that could be wished, may be abundantly evident to every person not obstinately blind to undeniable fact.

The thing is so self-evident as to require no proof, that, if both ends of the boat be tolerably filled with empty casks, she will not only thereby be secured against upsetting or sinking, but will be rendered extremely buoyant, provided the casks be effectually secured in their places; and in full proof of this fact, the experiment hereafter to be narrated was made almost entirely with empty casks.

The inventor having little hope that the far better and more eligible plan by timely preparation will be adopted, is the more solicitous to gain attention to this third mode, by means of casks only, because necessity, which is often the mother of persuasion as well as of invention, may compel the unfortunate mariner to have recourse to it.

Seamen being above all others expert in the use of ropes, and expeditious in making secure seizings, which is the great and only thing wanted, the inventor begs leave confidently to affirm, that whenever it shall be tried it will be found perfectly safe and successful.

Let therefore no scruple or hesitation be made in striking holes through the boat, any where, and of any number or size that may be found necessary for passing ropes for the effectual confinement of the casks. This plan will apply *not to one boat only, but to every boat in the ship*, provided there be a sufficiency of casks on board.

If then the two great points upon which I set out, namely, the powerful buoyancy of casks, and the peculiar expertness of seamen in every operation where ropes are to be used, be duly considered, they will sufficiently vindicate and

and verify all that I have stated; and unless the one or the other, or both, (that is, the power of casks, and expertness of seamen) can be shown to be false assumptions, the conclusions which I have drawn can neither be denied nor resisted.

Observations and Remarks relative to the foregoing Plans.

1.—From the detail in the description it may be alleged, that the situation would not admit of so much time as the preparation would require. Observations. Time.

It is granted, that in some cases this might be true, if nothing had been done before-hand; but surely such neglect ought by no means to be imputed as any defect in the plan, but ought to be ascribed to its true cause, the remissness of those who would give themselves no trouble to avail themselves of it.

Slings fitted to the casks, two additional ring bolts, two auger bores, and the requisite quantity of cork, are all things so trivial and so easy to be provided, that to be without them must appear an unpardonable neglect; and, if these were in readiness, the short space of ten minutes would be quite sufficient for laying them in their places, and securing them.

It is evident to demonstration, or it might be easily proved by experiment, with respect to the first two methods stated, and where the necessary provisions had been made, that the whole could be executed in ten minutes, and therefore any objection in point of time can have no place.

2.—When there is a prospect of the ship holding together for some time, the boat may be kept in readiness and in reserve, or may be served on shore by a rope, and hauled off again, as often as occasion may require; and if to be hauled off, it might be a necessary precaution to pass a rope round her lengthways to assist the ring bolt in the bow, and in every case the attachment and connection of the boat with the vessel ought to be well secured till the moment she is to be cast off for the shore. The boat may be served on shore by a rope and hauled off again.

May be got into the water any how.

3.—It is of no consequence in what manner this boat is to be got into the water, whether after-end or side, by means of handspikes or otherwise, as no water can hurt her; though it might be more desirable, if it could be done without filling her in midships, as in that case she might be conducted through very heavy seas without filling at all, or receiving more water than might be easily baled out.

No matter how deep the men are in water.

4.—It is material to remark, as it may not generally be attended to, that the plan always supposes the midships to be full of water; but that the requisite buoyancy of the boat is not injured by that circumstance, nor will the addition of people, in so far as they are immersed in the water, prove any additional burden; this will be perfectly clear to all who understand this part of the subject, however improbable it may appear to others, and the remark serves to show, that it would be a good rule, in such circumstances, for the men to keep themselves immersed in the water in midships as far as possible.

The idea of placing men in the midships of the boat, while at the same time it was full of water, would probably startle a landsman not a little; such therefore may be told, that every lifeboat is supposed full of water, and that to imagine there could be any man in one with a dry thread about him would argue a total ignorance of the matter.

5.—It is to be kept in mind, that the danger is always supposed to be extreme, and that the present plan affords the only possible chance of saving life; therefore whatever hardship or difficulty there may be in putting it in execution is entirely out of the question; any other view of the subject is altogether foreign to the purpose.

Casks superior to cork.

6.—If any are of opinion, that cork ought alone to be used for buoyancy, there can be no doubt of its answering the purpose perfectly; at the same time the author is of opinion, that a combination of cork and casks would be found more convenient, and in some respects preferable.

Water casks would always be at hand, and, to save the expense of cork, might on that account be preferred by some; but independent of this consideration, casks are by more than one half lighter than their bulk of cork, and thereby

thereby more than a double advantage in favour of buoyancy is gained by using them.

There is but one objection to the use of casks, and that is, that they may be stove in; but if the great strength which they possess from their construction be considered, and at the same time that they are strongly defended by the boat, this objection must appear of no moment at all.

7.—Every boat, prepared as has been stated, is fit to carry men equal in weight to something more than one third of the boat's whole burden, and one of eighteen feet in length can carry from fourteen to sixteen people, and have sufficient room for working a pair of oars, which ought by all means to be short ones. Proportion of men a boat will carry. Oars, if any, should be short.

The disadvantage of working long oars upon a low gunwale, and in a high running sea, is too obvious to need any thing more than to be just mentioned.

8.—As all depends upon the points of fixture, too much attention cannot be paid to their sufficiency, and though those stated in the plan are judged to be perfectly adequate to the purpose, yet any person, wishing for more, *may add them at pleasure*, by rings of rope in the stem and stern-posts, as in the Greenland boats; by more rings in the keel; or, in addition to the seizing ropes, a netting of small rope may be made to cover the whole foreward, and another such may be applied in the same manner aft, and by these means every possible security that can be desired may be obtained. The fixing should be secure.

9.—It is material to observe, that no dependence ought to be placed on seizings connected with the thwarts or gunwales, unless it were only as aids to the points of main dependence. The gunwales, more than any other part of the boat, are liable to damage, and may very possibly be injured in hoisting out, or before getting clear of the vessel. The thwarts & gunwales unsafe for this.

The two auger bores in the keel are infallible holds; easy access may be had to them while the boat is on deck, and a rope may be passed through them in a moment. This seizing, beside the security it affords for confining the buoyancy, adds considerably to the strength of the boat, and The auger holes in the keel secure.

and therefore ought to be preferred to any other mode of fixture.

10.—No rule can be laid down that will fit all boats, as to the precise quantity of cork, or size of casks, their shape and dimensions being so various; but from the general rule that has been stated, and the purpose to be served, every man may easily adjust his apparatus to his boat, or make such little alterations in the boat as may be found convenient or necessary.

A sail advantageous.

11.—No sail can hurt this boat, as it is supposed she has only to go right before the wind, and therefore a sail may be used with very great advantage. This would render oars unnecessary, and would be infinitely preferable. It is almost needless to add, that the boat could be steered in midships.

Disadvantages of the common lifeboat.

12.—The great benefit derived from the common lifeboats is well known, and universally acknowledged; but they are very far from being adequate to the calamity they are intended to remedy. Their number comparatively is very few, and the sphere of their operations extremely limited. In darkness by night, and in thick snow by day, when their aid is most wanted, they are of no avail. Storms may blow, and sometimes have blown so hard as to defeat their utmost exertions; and even in the most favourable cases, they require a considerable time before they can reach the wreck; in the mean time the vessel may be dashed to pieces, and all hands lost.

Superiority of the present.

The very preeminent advantage of the shipboat in these and several other respects is very conspicuous. This boat is wherever the ship is, and recourse may immediately be had to her; is of equal utility by night as by day, and in the thickest as well as in the clearest weather; and while the lifeboat, with extreme slow progress, must be impelled against wind and sea by a force superior to both, the shipboat has only to drift with ease before the storm.

The principle the same in both.

13.—As it may serve to gain confidence with those who are not otherwise qualified to judge of the plan, it may be observed, that the shipboat is prepared upon the very same principles as the lifeboat, and that these principles are applied to greater advantage in the former than in the latter.

The

The quantity of buoyancy in the shipboat, being considerably more in proportion to her size, and being carried to a greater height, gives more security against oversetting; and if to these advantages there be added the far greater one of having only to drift before wind and sea, no shadow of doubt remains of the success of the shipboat over that of the other.

Lastly.—This plan carries with it the very strong recommendation of private interest as well as of public utility. *The plan advantageous to private interest,*

Suppose a ship to be riding in an open bay or roadstead, a storm comes on, and, if in winter, a long dark night is soon to follow. In this situation the mariners, being extremely doubtful whether the vessel could hold it out over the night, and terrified at the awful prospect of being thrown, as it were, blindfold into the most perilous of all situations, the determination would most undoubtedly be to cut and let the ship run on shore while there was light, as giving the only chance for saving life.

The same determination may be taken in hopes of escaping by favour of a falling tide, and in both cases lives, ship, and cargo may be all lost, as has certainly very frequently happened. Whereas could safety be ultimately relied upon from the boat, the ship would be allowed to ride so long as anchors and cables could hold her; and in the mean time the storm might abate, the wind might shift, or her tackling might prove sufficient to ride out the storm, and thus lives, ship and cargo would all be safe. *as it would encourage men to stay by a ship in danger.*

In every situation the prospect of safety by means of the boat would prevent every precipitate measure, and encourage men to make those exertions for saving ship and cargo, which are not to be expected from men despairing of life.

In the foregoing plans there is nothing that can be reckoned complex, nothing that requires nice adjustment, or of doubtful and precarious effect. They are unquestionable in principle, simple and easy in execution, and absolute in security; and if the necessary previous preparation, which is very little, has been made, they will be found as expeditious as any emergency can require. They have been proved by experiment as far as circumstances would permit, *It is simple, & unobjectionable.*

mit, and have received the unqualified approbation of naval men of the greatest experience, and of the first respectability.

These are the solid grounds upon which they are offered to the public in general, and most earnestly pressed upon the attention of seamen in particular.

The plan having been commmunicated to hundreds of seafaring men, they have always given it their ready and entire approbation; hence it is hoped, that every seaman from his own knowledge and experience, without any doubt whatever, will, upon considering the subject, be fully convinced in his own mind, that the scheme is perfectly practicable, and if adopted, would be attended with the happiest effect.

Substitutes and Expedients which may be found useful.

Substitutes. 1.—If so much cork was made up in canvas as would serve to go quite round the boat withoutside, and reach from the top of the gunwales to about fifteen inches downward, and of one foot in thickness; the same might be attached to the boat, and would render her extremely buoyant. This, together with ballast, and a small quantity of cork withinside, would produce a perfect lifeboat, upon almost the very same plan as the present lifeboats.

The cork might be made up in so many separate parcels, (netted in small rope,) as was found convenient to be attached to a strong rope going round the gunwale, and to another such which ought exactly to fit the girth of the boat where the cork reached to below.

As the cork would only press upwards, and always against the bottom and sides of the boat, it is evident, that, if the lower rope fitted tightly, the cork would keep its place; and in order to secure that point a few turns of rope passing from the lower edge on the one side over the keel to the lower edge on the other side would fix it completely. A very few seizings attached to the gunwale rope passing from the one side to the other would be quite sufficient.

The separate parcels must be furnished with loops or ends for attaching them to the main ropes, and to one another.

2.—Several

2.—Several ringbolts might be put into the keel withinside, and ropes, single, double, or treble, might be passed through these rings before laying in the buoyant materials, and then these ropes might be brought round the whole contents and made fast.

3.—It frequently happens that seamen, after they have gained the shore, find they have only escaped one death to perish by another still more miserable.

Drenched in water, chilled to the heart with cold, worn out with fatigue, and exposed to all the severity of inclement weather, without shelter or succour, it is impossible but that the remains of life must soon be extinguished. Dry clothing

In this situation, and it is far from being uncommon, dry clothing would be as precious as life itself, and it might be had by the following expedient:—

Let a *leathern* bag be made for containing some shirts, a waistcoat or two, and two pair of drawers, all of flannel.

Let this bag be made of a length and size convenient for the purpose, and for tying round under the arm-pits.

This would serve the purpose of a cork-jacket in the water, and prove a second time as life from the dead, by affording dry and warm clothing upon gaining the shore. made to answer the purpose of a cork jacket.

By this expedient every man may be made a swimmer, and sometimes one man by smimming has been the means of saving the whole ship's company.

It may be proper to observe, that the larger part of the bag should be placed high upon the breast, and the other or back part no higher than the armpits, as in the act of swimming the back part of the shoulders is little more than just covered with the water.

The bag must be perfectly water-tight, and only moderately filled; both ends of it may be left open to be closed with a tight seizing of small line.

The expense of this preparation would hardly be five shillings.

4.—If it were intended only for swimming, a neat and commodious preparation might be made with cork covered with thin leather*, to be applied in the manner which has just been described.

* I conceive flannel would be preferable. It would be less cold, and not so much affected by soaking in water. C.

It

It might be fitted on with clothes or without in half a minute, and made fast by a knot or clasp on the breast; three pounds of good cork would be sufficient to support any man, and the expense no more than in the former case.

A line might be conveyed on shore by a kite

5.—Another expedient bids fair for obtaining a speedy communication betwixt the ship and the shore, by means of a kite.

It is the property of this machine, to ascend in proportion as the cord is spared off.

To manage this, and to bring the line within reach on shore, let a piece of light wood, about the size of a small handspike, be attached to the line, about twelve fathoms from the kite; the line to be fixed to the forepart of the stick, and so as to pull only there, and then being slackly laid along the stick, made fast to the other end: by this means the kite would be prevented from rising higher, and would, at the same time bring the line to the shore from the ship, and by this small line a rope might be hauled from the ship by any spectator on the shore.

A silk handkerchief, and a piece of wooden hoop, might soon furnish a kite.

or by a spread ensign.

6.—Sometimes people are seen to perish, where those on shore, and those on the wreck, are almost within grasp of each other.

communicated to Lord Melville, when Treasurer of the Navy, in 1792: whether the lifeboat were borrowed from it is more questionable.

Mr. Bremner also takes this opportunity of asserting his claim to the invention of applying locks to cannon, which he communicated to the late Sir Charles Douglas so long ago as the year 1768. and of locks to cannon.

III.

On the Scale of the Barometer, and the Construction of an Airpump for procuring a perfect Vacuum. In a Letter from a Correspondent.

To Mr. NICHOLSON.

SIR,

I FEEL much obliged by the insertion of my paper on the Airpump, in your very valuable Journal. Should the following hint, respecting the construction of the barometer, (which is at least new to myself,) appear to be worthy the attention of your readers, it is much at your service.

In Mr. Dalton's Meteorological Observations, page 7, where he is speaking of the barometer, I find the following remark: "The scale in strictness ought not to be full "inches, but something less, owing to the rising and falling "of the surface of the reservoir. If the tube have a bulb, "then the area of the surface at the top of the column, divided by the sum of the areas of the top and reservoir, "will give the part to be deducted; but if the tube be "straight, then the whole area of the reservoir, lessened by "the area of the glass annulus, made by a horizontal section of the erected tube, must be used as the denominator of the fraction; hence, if the fraction be $\frac{1}{60}$, then "the scale of 3 inches must be diminished by half a tenth." At page 9 the following observation occurs. "With respect to the barometers at Kendal and Keswick, they "were both clear of air and moisture, and exhibited the "electric light in the dark. The scales were both *full* "inches, and therefore the variations were somewhat greater "than Observations of the scale of the barometer.

.) It may not be amiss to observe here, that it seems necessary, that a correction for this should be made, in taking observations with the barometer; as the mercurial atmosphere will react upon the surface of the top of the column, and prevent it from rising to the full height, to which it would otherwise attain.

Airpump for a perfect vacuum,

By the following means, a *perfect vacuum*, I believe, may be obtained. Let A B (fig. 2) be a tube of metal, ground so as to be perfectly cylindrical in the inside: let C C be the piston-rod; and D the piston, which is solid; and let *a* be a small metallic valve, opening outwards: also let the concave and convex surfaces of the barrel, and of the piston, be ground accurately to each other. Let us imagine the piston to be at the top of the barrel, and that all the air is expelled by means of the valve *a*: if the piston be now forced downwards, the space above it will be a *perfect vacuum*; at least with respect to air, and all evaporable fluids. The valve might be placed in the piston, as represented by the dotted lines at *b*; and in this case the one at *a* would be unnecessary. The action of this pump might be rendered more secure, by closing the bottom of the barrel, and inserting in it a metallic valve, opening outwards; and by making the piston rod to move in a collar of leathers: the piston also and the bottom of the barrel might be ground to each other. By these means, there would be but little danger of any air forcing itself into the vacuum, which would be *perfect* above the piston, and nearly, if not quite, so below it. This addition is shown by the dotted lines. If the barrel were made of glass, we should then have it in our power, to observe the appearance of the electric fluid, in a *perfect vacuum*; which, I believe, has never yet been the case. The bottom of the piston also might be formed of different metals, and exposed to the action of a burning glass, or of a galvanic battery. If any elastic fluid were generated by the process, it would be easy to collect and to ascertain its nature. Many other solid substances might be acted upon, by fixing them accurately into the bottom of the piston, so as to form a part of it; and by using the same agents, as in the former case.

in which some experiments might be made.

The principle upon which this pump is constructed I consider

consider as perfect; its application is by no means so: it may nevertheless be a useful hint to those, who are engaged in very refined experiments on the nature of the metals, &c. It would be less troublesome to construct, and perhaps more applicable to most purposes, if made after the following plan. Let A B be a glass cylinder, D the piston, and E E a plate of glass, large enough to cover the top of the barrel. When this instrument is to be made use of, let the piston be forced upwards, so as to project about a line or less above the cylinder; let the glass plate E E (which must be well ground to the top of the piston) be laid upon the piston, and let it then be drawn downwards; the plate will be kept in its place, by the pressure of the atmosphere, (or other means may be made use of to keep it more secure) and the vacuum will be perfect, as in the former case. The glass E E may be made of a piece of common plate glass, if truly ground; and the focus of the lens will easily be directed through it, so as to fall upon the bottom of the piston. Another construction for the same purpose.

I am, Sir,

Your obliged and constant reader,

L. O. C.

IV.

A Description of a Forcing House for Grapes; with Observations on the best Method of constructing them for other Fruits. By T. A. KNIGHT, *Esq. F. R. S. &c.**

SO much difference of opinion prevails among gardeners respecting the proper forms of forcing houses, that two are rarely constructed quite alike, though intended for the same purposes; and every gardener is prepared to contend, that the form he prefers is the best, and to appeal to the test of successful experiment, in support of his opinion. And this he is generally enabled in some degree to do, because plants, when properly supplied with food, and water, and heat, will succeed in houses, the forms of which are very defective; and proper attention is not often paid Construction of forcing houses for fruit.

* Trans. of the Horticultural Soc. Vol. 1, p. 99.

by

discovered many years ago, and have always practised with success: if to 4 parts of scraped cheese be added one part of calcined oyster shells, or other pure calcareous earth, and this composition be pressed strongly into the pores of the wood, the sap will instantly cease to flow; the largest branch may of course be taken off at any season with safety.

V.

On some of the Combinations of Oximuriatic Gas and Oxigen, and on the Chemical Relations of these Principles to Inflammable Bodies. By HUMPHREY DAVY, *Esq. LL. D. Sec. R. S. Prof. Chem. R. I. F. R. S. E.**

1. *Introduction.*

Oximuriatic acid gas a simple substance.

IN the last communication which I had the honour of presenting to the Royal Society, I stated a number of facts, which inclined me to believe, that the body improperly called in the modern nomenclature of chemistry *oximuriatic acid gas* has not as yet been decomposed; but that it is a peculiar substance, elementary as far as our knowledge extends, and analogous in many of its properties to oxigen gas.

My objects in the present lecture are to detail a number of experiments, which I have made for the purpose of illustrating more fully the nature, properties, and combinations of this substance, and its attractions for inflammable bodies, as compared with those of oxigen; and likewise to present some general views and conclusions concerning the chemical powers of different species of matter, and the proportions in which they enter into union.

I have been almost constantly employed, since the last session of the society, upon these researches, yet this time has not been sufficient to enable me to approach to any thing complete in the investigation. But on subjects, important

* Phil. Trans. for 1811, P. 1.

both

both in their connexion with the higher departments of chemical philosophy, and with the œconomical applications of chemistry, I trust that even these imperfect labours will not be wholly unacceptable.

2. *On the Combinations of Oximuriatic Gas and Oxigen with the Metals from the fixed Alkalis.*

The intensity of the attraction of potassium for oximuriatic gas is shown by its spontaneous inflammation in this substance, and by the vividness of the combustion. I satisfied myself, by various minute experiments, that no water is separated in this operation, and that the proportions of the compound are such, that one grain of potassium absorbs about 1·1 cubical inch of oximuriatic gas at the mean temperature and pressure, and that they form a neutral compound, which undergoes no change by fusion. I used, in the experiments from which these conclusions are drawn, a tray of platina for receiving the potassium; the metal was heated in an exhausted vessel, to decompose any water absorbed by the crust of potash, which forms upon the potassium during its exposure to the atmosphere, and the gas was freed from vapour by muriate of lime. Large masses of potassium cannot be made to inflame, without heat, in oximuriatic gas. In all experiments in which I fused the potassium upon glass, the retorts broke in pieces, in consequence of the violence of the combustion, and even in two instances when I used the tray of platina. If oximuriatic gas be used not freed from vapour, or if the potassium has been previously exposed to the air, a little moisture always separates during the process of combustion. When pure potassium, and pure oximuriatic gas are used, the result, as I have stated, is a mere binary compound, the same as muriate of potash, that has undergone ignition.

Potassium inflames in oximuriatic gas, and forms a neutral compound unalterable by fusion, the same as muriate of potash

The combustion of potassium and sodium in oxigen gas is much less vivid than in oximuriatic gas. From this phenomenon, and from some others, I was inclined to believe, that the attraction of these metals for oxigen is feebler, than their attraction for oximuriatic gas. I made several experiments, which proved that this is the fact; but before I enter upon a detail of them, it will be necessary to discuss more

Potassium and sodium burn less vividly in oxigen than in oximuriatic gas.

more fully, than I have yet attempted, the nature of the combinations of potassium and sodium with oxigen, and of potash and soda with water.

When this is done on platina, the metal oxided.

I have stated in the last Bakerian Lecture, that potassium and sodium, when burnt in oxigen gas, produce potash and soda in a state of extreme dryness, and very difficult of fusion. In the experiments from which these conclusions are drawn, as I mentioned, I used trays of platina, and finding that this metal was oxidated in the operation, I heated the retort strongly, to expel any oxigen the platina might have absorbed, and, except in cases when this precaution was taken, I found the absorption of oxigen much greater than could be accounted for by the production of the alkalis. In all cases in which I burnt potassium or sodium in common air, applying only a gentle heat, I found that the first products were substances extremely fusible, and of a reddish brown colour, which copiously effervesced in water, and which became dry alkali, by being strongly heated upon platina in the air; phenomena, which, at an early period of the inquiry, induced me to suppose that they were protoxides of potassium and sodium. Finding, in subsequent experiments, however, that they deflagrated with iron filings, and rapidly oxidated platina and silver, I suspended my opinion on the subject, intending to investigate their nature more fully.

Potassium and sodium burned in common air produce brown, fusible substances,

which are peroxides.

Since that time, these oxides, as I find by a notice in the Moniteur for July 5th, 1810, have occupied the attention of Messrs. Gay-Lussac and Thenard; and these able chemists have discovered, that they are peroxides of potassium and sodium, the one containing, according to them, three times as much oxigen as potash, and the other 1·5 times as much as soda.

When they are formed on metallic substances these are always oxided.

I have been able to confirm in a general way these interesting results, though I have not found any means of ascertaining accurately the quantity of oxigen contained in these new oxides. When they are formed upon metallic substances, there is always a considerable oxidation of the metal, even though platina be employed. I have used a platina tray lined with muriate of potash, that had been fused; but in this case, though I am inclined to believe that

that some alkali was formed at the same time with the peroxides, yet I obtained an absorption of 2·6 cubical inches, in a case when 2 grains of potassium were employed, and of 1·63 cubical inches, in a case when a grain of sodium was used, but in this last instance the edge of the platina tray had been acted upon by the metal, and was oxidated*. The mercury in the barometer in these experiments stood at 30·12 inches, and that in the thermometer at 62° Fahrenheit.

When these peroxides were formed upon muriate of potash, the colour of that from potassium was of a bright orange; that from sodium of a darker orange tint. They gave off oxigen, as Messrs. Gay-Lussac and Thenard state, by the action of water or acids. They were converted into alkali, as the French chemists have stated, by being heated with any metallic or inflammable matter. They thickened fixed oils, forming a compound, that did not redden paper tinged with turmeric, without the addition of water. Their properties.

When potassium is brought into contact with fused nitre, in tubes of pure glass, there is a slight scintillation only, and the nitre becomes of a red brown colour. In this operation, nitrogen is produced, and the oxide of potassium formed. I thought that by ascertaining the quantity of nitrogen evolved by the action of a given weight of potassium, and comparing this with the quantity of oxigen disengaged from the oxide by water, I might be able to determine its composition accurately. A grain of potassium acting in this way, I found, produced only 0·16 of nitrogen; and the red oxide, by its action upon water, produced less than half a cubical inch of oxigen, so that it is probable, that potash as well as its peroxide is formed in the operation. Action of potassium on fused nitre.

Sodium, when brought into contact with fused nitre, Action of so-

* Messrs. Gay-Lussac and Thenard have stated in the paper above referred to, that common potash and barytes absorb oxigen when heated. It would seem, that the action of the fixed alkalis and of barytes on platina depends on the production of the peroxides. I have little doubt, but that these ingenious gentlemen will have anticipated this observation, in the detailed account of their experiments. Potash and barytes absorb oxigen when heated.

 produced

dium on fused nitre.

produced a violent deflagration. In two experiments in which I used a grain of the metal, the tube broke with the violence of the explosion. I succeeded in obtaining the solid results of the deflagration of ½ a grain of sodium; but it appeared, that no peroxide had formed, for the mass gave no oxigen by the action of water.

Potassium burned in a retort of pure glass,

When potassium is burnt in a retort of pure glass, the result is partly potash and partly peroxide, and by a long continued red heat the peroxide is entirely decomposed.

and in one of green glass, containing oxigen.

A grain of potassium was gently heated in a small green glass retort containing oxigen; it burnt slowly, and with a feeble flame; a quantity of oxigen was absorbed equal to 0·9 of a cubical inch; by heating the retort to dull redness, oxigen was expelled equal to 0·38 of a cubical inch; the mercury in the thermometer in this experiment stood at 63° Fahrenheit, and that in the barometer at 30·1 inches.

Electrical decomposition of potash and soda.

In experiments on the electrical decomposition of potash and soda, when the Voltaic battery employed contains from 500 to 1000 series in full action; the metals burn at the moment of their production, and form the peroxides; and it is probable, from the observations of Mr. Ritter, that these bodies may be produced likewise in Voltaic operations on potash, at the positive surface.

Supposed protoxides.

In my early experiments on potassium and sodium, I regarded the fusible substances appearing at the negative surface, in the Voltaic circuit, as well as those produced by the exposure of the metals to heat and air, as protoxides, and as similar to the results obtained by heating the metals in contact with small quantities of alkali.

I have repeated these last operations, in which I conceived that protoxides were formed.

Potassium and sodium, when heated in glass tubes in contact with about half of their weight of potash and soda, that have been ignited, become first of a bright azure, then produce a considerable quantity of hidrogen, and at last form a gray coherent mass, not fusible at a dull red heat, and which gives hidrogen by the action of water.

Whether these are true protoxides, or merely mixtures of the alkaline metals with the alkalis, or with the alkalis and reduced

reduced silex from the glass, I shall not at present attempt to decide.

Potassium I find heated in a similar manner with fused potash, in a tube of platina, gives, after having been ignited, a dark mass that effervesces with water; but even in this case, it may be said, that the alloy of platina and potassium interferes, and that the substance is not a protoxide, but merely dry alkali mixed with this alloy.

As the pure alkalis were unknown, till the discovery of potassium and sodium*, and as their properties have never been described, it will perhaps be proper in this place to notice them briefly.

Description and properties of the pure alkalis.

When potassium and sodium are burnt in oxigen gas upon platina, and heated to redness to decompose the peroxide of potassium, the alkalis are of a grayish green colour. They are harder than common potash or soda, and, as well as I could determine by an imperfect trial, of greater specific gravity. They require a strong red heat for their perfect fluidity, and evaporate slowly, by a still farther increase of temperature. When small quantities of water are added to them, they heat violently, become white, and are converted into hydrats, and then are easily fusible and volatile.

When potassium or sodium is burnt on glass, freed from metallic oxides, and strongly heated, or when potash or soda is formed from the metals by the action of a minute quantity of water, their colour approaches to white; but in other sensible properties they resemble the alkalis formed upon metallic substances; and are distinguished in a marked

Stahl nearly discovered the pure alkalis.

* Stahl approached nearly to the discovery of the pure alkalis He cemented solid caustic potash with iron filings in a long continued heat, and states, that in this way an alkali "valde causticum" is produced. *Specim. Bech.* part ii, p. 255. He procured caustic alkali also, by decomposing nitre by the metals. Id. p. 253.

Affinity of potash for metallic oxides.

I find, that, when nitre is decomposed in a crucible of platina by a strong red heat, a yellow substance remains, which consists of potash and oxide of platina, apparently in chemical combination. The undecompounded potash, which comes over in the process for procuring potassium by the gunbarrel, is of an olive colour, and affords oxide of iron during its solution in water. Pure potash will probably be found to have an affinity for many metallic oxides.

manner

manner by their difficult fusibility from the potash and soda prepared by alcohol.

Water in the alkalis.

Mr. D'Arcet, and more distinctly Mr. Berthollet, have concluded, that the loss of weight of common fused potash and soda, during their combination with acids, depends upon the expulsion of water, which Mr. Berthollet has rated at 13·9 per cent for potash, and Mr. D'Arcet, at 27 or 28 for potash, and 28 or 29 for soda *.

I have stated in the last Bakerian lecture, that my own results led me to conclude, that fused potash contained about 16 or 17 parts in the 100 of water, taking the potash formed by adding oxigen to potassium as a standard.

The experiment, from which I drew my conclusions, was made on the action of silex and potash fused together, and I regarded the loss of weight as the indication of the quantity of moisture.

Water not yet collected from the ignited alkalis.

I am acquainted with no experiment on record, in which water has been actually collected from the ignited fixed alkalis, and this appeared necessary for the complete elucidation of the subject.

Experiment to effect this with potash.

I heated together, in a green glass retort, 40 grains of potash, (that had been ignited for several minutes), and 100 grains of boracic acid, which had been heated to whiteness for nearly an hour. The retort was carefully weighed, and connected with a small receiver, which was likewise weighed; the bulb of the retort was then gradually heated till it became of a cherry red; there was a violent effervescence in the retort, a fluid condensed in the neck, and passed into the receiver. When the process was completed, the whole of the retort was strongly heated; it was found to have lost 6¼ grains, and the receiver had gained 5·8 grains. The fluid that it contained was water, holding in solution a minute quantity of boracic acid, and when evaporated, it did not leave an appreciable quantity of residuum.

Water 0·17? or 0·19?

A similar experiment made upon soda, heated to redness, but in which the water collected was not weighed, indicated 22·9 of water in 100 parts of soda.

Water from soda 0·23.

* Annales de Chimie, tom. 68, page 190; or Journal, vol. XXVII, page 31.

It

It may be asked, whether part of the water evolved in these processes might not have been produced from the boracic acid, or formed in consequence of its agency; but the following experiments show, that this can not be the case in any sensible degree. None of the water from the boracic acid.

I heated 8 grains of potassium, with about 50 grains of boracic acid, to redness in a tube of platina, connected with a glass tube, kept very cool; but I found that no moisture whatever was separated in the process. I mixed a few grains of potassium with red oxide of mercury, and ignited the mixture in contact with boracic acid, but no elastic product, except mercury, was evolved. Proofs.

I made some potash by the combustion of potassium in a glass tube, and ignition of the peroxide; I added to it dry boracic acid, and heated the mixture to redness. Subborate of potash was formed, and there was not the slightest indications of the presence of moisture *.

It

* These processes must not however be considered as showing, that boracic acid that has been heated to whiteness is entirely free from water; they merely prove, that such an acid gives off no water by combination with pure potash at a *red heat*. I have found, that boracic acid in perfect fusion, and that has been long exposed to the blast of a forge, and that has long ceased to effervesce, gives globules of hidrogen, when dry iron filings are made to act upon it. I added to 54 grains of boracic acid in complete fusion, in a crucible of platina, 75 grains of flint glass that had been previously heated to whiteness, and immediately reduced into powder in a hot iron mortar; by raising the heat so as to produce combination, a copious effervescence was produced; and after intense ignition for half an hour, the mixture was found to have lost three grains and a quarter. Boracic acid heated to whiteness not free from water.

The combinations of boracic acid with potash and soda, that have been heated to redness, I find lose weight when their temperature is raised to a much higher degree. Thus, in an experiment made in the laboratory of my friend John George Children, Esq., and in which Mr. Children was so kind as to cooperate, 71 grains of hydrat of potash, mixed with 96 of boracic acid that had been heated as strongly as possible in a blast furnace, lost by fusion together in a red heat 11 grains, but on raising the temperature to whiteness the loss increased to above 13 grains. 55·5 grains of hydrat of soda, mixed with 80 of boracic acid, examined at intervals in a process of this kind, continued to lose weight for half an hour, during which time they were frequently heated to whiteness; at the end of this period the whole loss was 14 grains, of which at least one grain and a half

may

The common alkalis hydrats.

It is evident from this chain of facts, that common potash and soda are hydrats, and the bodies formed by the combustion of the alkaline metals are, as I have always stated, pure metallic oxides, (as far as our knowledge extends) free from water*.

I shall

may be referred to the acid. 95 grains of soda, ignited to whiteness in a platina crucible, with 140 of dry flint glass, lost 22·2 grains; 80 grains of boracic glass were added to this mixture; a fresh effervescence took place, and after intense ignition for a few minutes, there was an additional loss of weight of four grains and a half. The energy with which water adheres to certain bodies in other cases is shown by the experiments of Mr. Berthollet, *Mém. d'Arcueil*, tom. ii, page 47. Indeed it is impossible to say, that a neutral compound, or a fixed acid, is ever entirely free from water; it is only the first proportions that are easily separated. If the proportions of water in common potash and soda were to be judged of from their loss of weight, in combining with boracic acid, it would appear to be from 19 to 20 per cent in the first, and from 23 to 25 in the second.

Potassium and sodium not hydrurets.

* After the experiments detailed in my last two papers, it may perhaps appear unnecessary, at least to those enlightened British chemical philosophers, who have closely followed the progress of science, to offer any new evidences to prove, that potassium and sodium are not hydrurets of potash and soda; particularly as Messrs. Gay-Lussac and Thenard, the ingenious advocates of this notion, have acknowledged, in the Moniteur to which I have before referred, that it is not tenable; but on a subject so intimately connected with the most refined departments of chemical philosophy, and with so many new objects of research, additional facts cannot be wholly devoid of use and application.

Mr. Dalton, in the second volume of the work which he entitles "*A New System of Chemical Philosophy*," of which he has had the goodness to send me a copy, has, I find in his first pages, adopted the idea, that potash and soda are metallic oxides; but in the latter pages has considered them as simple bodies, and the metals formed from them as compounds of potash and soda with hidrogen. He has given no facts in favour of this change in his opinion: his principal argument is founded upon the process in which I first obtained potassium. Common potash is a hydrat: when oxigen is procured from this by Voltaic electricity at one surface, and potassium at the other surface; Mr. Dalton, conceiving that this oxigen arises from the water, states, that the hidrogen of the water must combine with the potash to form potassium. It is evident, that, adopting such a plan of reasoning, lead and copper might be proved to be hydrurets of their oxides; for when these metals are revived from their aqueous acid solutions, oxigen is produced at the positive surface, and no hidrogen at the negative surface,

In

I shall now resume the detail of the experiments that I have made, on the relative attractions of oximuriatic gas and

Potassium burned in oxigen,

In my first experiments for producing potassium and sodium, I used a weak power; and in these instances, procuring the metals in very small quantities only, I perceived no effervescence. When from five hundred to one thousand plates are used for producing potassium, there is a violent effervescence, and a production of hidrogen, and sometimes of potassuretted hidrogen, connected with the formation of the metal.

In the production of potassium in quantity hidrogen evidently evolved,

Potassium, brought into contact with redhot hydrat of potash, disengages abundance of hidrogen, and the whole is converted into difficultly fusible potash.

as it is in other experiments.

327 grains of hydrat of potash, that had been ignited, were made to act in a gunbarrel on 745 grains of iron turnings heated to whiteness. Some hidrogen was lost, and some hydrat of potash remained undecompounded, yet 225 cubical inches of inflammable gas were collected, and 50 grains of potassium, and a large quantity of an alloy of potassium and iron formed; so that it is scarcely possible to doubt, that all the hidrogen produced from the decomposed hydrat of potash was liberated.

Mr. Dalton conceives, that there is an analogy between potassium and sodium, and the compounds of hidrogen with sulphur, phosphorus, and arsenic; but I am at a loss to trace any similarity between sulphuretted hidrogen, which is a gaseous body, soluble in water, and having acid properties, and a highly inflammable solid metal, which produces alkali by combustion. Potassium might as well be compared to carbonic acid. Mr. Dalton considers the volatility of potassium and sodium as favouring the idea of their containing hidrogen; but they are less volatile than antimony, arsenic, and tellurium, and much less volatile than mercury. He mentions their low specific gravity as a circumstance favourable to this idea. I have on a former occasion examined this argument, first brought forward by Mr. Ritter; but it may not be amiss to add, that, if potassium is a compound of hidrogen and potash, hydrat of potash must contain an equal quantity of hidrogen, with the addition of a light gaseous element, oxigen, which might be expected to diminish rather than to increase the specific gravity of the compound. Mr. Dalton states, p. 488, that potassium forms dry hydrat of potash, by decomposing nitrous gas and nitrous oxide; this is not the case: and he does not refer to experiment. I find by some very careful trials, that potassium attracts the oxigen and some of the nitrogen from these bodies, and forms a fusible compound which may be decomposed, giving off nitrogen and its excess of oxigen, by a red heat, and which becomes *potash*, and not dry hydrat of potash.

Observations on some opinions of Mr. Dalton's.

Messrs.

and oxigen for the metals of the fixed alkalis. I burnt a grain of potassium in oxigen gas, in a retort of green glass, furnished with a stopcock, and heated the oxide formed to redness, to convert it into potash: half a cubical inch of oxigen was absorbed. The retort was exhausted, and very pure oximuriatic gas admitted. The colour of the potash instantly became white; and by a gentle heat the whole was converted into muriate of potash: a cubical inch and $\frac{1}{4}$ of oximuriatic gas were absorbed, and exactly half a cubical inch of oxigen generated. The barometer during this operation was at 30·3, the thermometer at 62 Fahrenheit. I made several experiments of the same kind, but this is the only one on which I can place entire dependence. When I attempted to use larger quantities of potassium, the retort usually broke during the cooling of the glass, and it was not possible to gain any accurate results in employing metallic trays. The potassium was spread into a thin plate, and of course was much oxidated before its admission into the retort, which rendered the absorption of oxigen a little less than it ought to have been. In the process it was heated in vacuo before the combustion, to decompose the water in the crust of potash; for in cases when this precaution was not taken, I found that hydrat of potash sublimed, and lined the upper part of the retort, and from this the oximuriatic gas separated water as well as oxigen.

and oximuriatic gas admitted.

Muriate of potash formed, and all the oxigen given out.

Water separated from hydrat of potash by oximuriatic gas.

The phenomenon of the separation of water from hydrat of potash by oximuriatic gas was happily exemplified in an experiment in which I introduced oximuriatic gas to the peroxide of potassium, formed in a large retort, and in which the potassium had been covered with a considerable crust of hydrat of potash. The upper part of the retort and its neck contained a white sublimate of hydrat, which had risen in combustion, and which was perfectly opaque. As soon as the gas was admitted, it instantly became transparent from the evolution of water; and on heating the glass in

Messrs. Gay-Lussac and Thenard have convinced themselves, that potassium and sodium are not hydrurets of potash and soda, by a method similar to that which I adopted and published some months before, namely, by producing neutral salts from them.

in contact with the sublimate, its opacity was restored, and water driven off.

In various cases in which I heated dry potash, or mixtures of potash and the peroxide, in oximuriatic gas, there was no separation of moisture, except when the gas contained aqueous vapour; and the oxigen evolved in the process, when the heat was strongly raised, exactly corresponded to that absorbed by the potassium.

Decomposition of muriatic acid gas by potash,

When muriatic acid gas was introduced to potash formed from the combustion of potassium, water was instantly formed, and oximuriate of potassium *. I have made no accurate experiment on the proportions of muriatic acid gas decomposed by potash, but I made a very minute investigation of the nature of the mutual decomposition of this substance and hydrat of potash.

and by hydrat of potash.

Ten grains of hydrat of potash were heated to redness in a tray of platina, which was carefully weighed; it was introduced into a retort which was exhausted of air, and the retort was filled with muriatic acid gas. The hydrat of potash was heated by a spirit lamp; water instantly separated in great abundance, and muriate of potash formed. A strong heat was applied till the process was completed, when the tray was taken out and weighed; it had gained $2\frac{1}{10}$ grains. A minute quantity of liquid muriatic acid was added to the muriate, to ensure a complete neutralization, and the tray heated to redness: there was no additional increase of weight.

Action of sodium and soda on oximuriatic gas.

In the few experiments which I have made on the action of sodium and soda on oximuriatic gas, the phenomena appeared precisely analogous; but sodium, as might have been expected, absorbed nearly twice as much oximuriatic gas as potassium.

Muriate of soda decomposed by potassium.

When common salt, that has been ignited, is heated with potassium, there is an immediate decomposition, and by giving the mixture a red heat, pure sodium is obtained; and this process affords an easy mode, and the one I have always lately adopted, for procuring that metal. No hidrogen is disengaged in this operation, and two parts of

* i. e. Muriate of potash.

potassium

potassium I find produce rather more than one of sodium.

The experiments agreeable to calculation.

From the series of proportions that I have communicated in my last paper, it is evident, that 1 grain of potassium ought to absorb 1·08 cubical inches of oximuriatic acid; and that the potash formed from one grain of potassium ought to decompose about 2·16 cubical inches of muriatic acid gas; and these estimations agree very nearly with the result of experiments.

The estimation of the composition of soda, as deduced from the experiments in the last Bakerian lecture, is 25·4 of oxigen to 74·6 of metal, and this would give the number representing the proportion in which sodium combines with bodies 22*; from which it is evident, that a grain of sodium ought

* Or, if soda be considered as deutoxide, which seems probable from the experiments detailed page 114, 44; and on this supposition, the salts of soda must be conceived to contain double proportions of acid. On either datum the proportion of oxigen in water must be taken as 7·5, and that of hidrogen as 1, though other numbers might be found as divisors or multiples of these, which would equally harmonise with the general doctrine of definite proportions. In my last communication to the Society, I have quoted Mr. Dalton as the original author of the hypothesis, that water consists of 1 particle of oxigen, and 1 of hidrogen; but I have since found, that this opinion is advanced in a work published in 1789, *A comparative View of the Phlogistic and Antiphlogistic Theories*, by William Higgins. In this elaborate and ingenious performance Mr. Higgins has developed many happy sketches of the manner in which (on the corpuscular hypothesis) the particles or molecules of bodies may be conceived to combine; and some of his views, though formed at this early period of investigation, appear to me to be more defensible, assuming his data, than any which have been since advanced; for instance, he considered nitrous gas as composed of two particles of oxigen, and one of nitrogen. Mr. Higgins had likewise drawn the just conclusion respecting the constitution of sulphuretted hidrogen, from its electrical decomposition. As hidrogen is the substance which combines with other bodies in the smallest quantity, it is perhaps the most fitted to be represented by unity; and on this idea the proportions in ammonia will be 3 of hidrogen to 1 of nitrogen, and the number representing the smallest proportion in which nitrogen is known to combine will be 13·4. Mr. Dalton, *New System of Chemical Philosophy*, pages 323 and 436, has adopted 4·7 or 5·1, as the number representing the weight of the *atom* of nitrogen; and has quoted my experiment, *Researches, Chemical and Philosophical*, as authorising these numbers; but all the inquiries on nitric acid, nitrous gas, nitrous

Hypothesis of Mr. Higgins.

Remarks on some of Mr. Dalton's.

ought to absorb nearly 2 cubical inches of oximuriatic gas; and that the same quantity, converted into soda, would decompose nearly four cubical inches of muriatic gas. Muriate of soda ought on this idea to contain one proportion of sodium, 22, and one of oximuriatic gas 32·9; and this estimation is very near that which may be gained from Dr. Marcet's analysis of this substance. Hydrat of potash ought to consist of 1 proportion of potash, represented by 48, and one of water, represented by 8·5. This gives its composition as 15·1 of water, and 84·9 of potash. Hydrat of soda ought, according to theory, to contain 1 proportion of soda 29·5, and 1 of water 8·5, which will give in 100 parts 22·4 of water;

nitrous oxde, and on the decomposition of nitrat of ammonia stated in that work, conform much more nearly to the number 13·4.

According to Mr. Dalton, nitrate of ammonia contains one proportion of acid and one of alkali, and nitrate of potash two proportions of acid and one of alkali; but it is easy to see, that the reverse must be the case. Nitrate of ammonia is known to be an acid salt; and nitrate of potash a neutral salt; which harmonizes with the views above stated. Mr. Dalton estimates the quantity of water in nitric acid of specific gravity 1·54, at 27·5 per cent; and this, according to him, is a stronger acid than he obtained by decomposing fused nitre by sulphuric acid, which contained only 19 per cent of water; and one quantity of sulphuric acid, according to him, will produce from nitre more than an equal weight of nitric acid, and he supposes no water in nitre; so that his conclusion as to the quantity of water in liquid nitric acid on his own data must be incorrect. I find water in fused nitre, by decomposing it by boracic acid.

I shall enter no farther at present into an examination of the opinions, results, and conclusions of my learned friend; I am however obliged to dissent from most of them, and to protest against the interpretations that he has been pleased to make of my experiments; and I trust to his judgment and candour for a correction of his views.

Hypothesis of definite proportions.

It is impossible not to admire the ingenuity and talent, with which Mr. Dalton has arranged, combined, weighed, measured, and figured his atoms; but it is not, I conceive, on any speculations upon the ultimate particles of matter, that the true theory of definite proportions must ultimately rest. It has a surer basis in the mutual decomposition of the neutral salts, observed by Richter and Guyton de Morveau, in the mutual decompositions of the compounds of hidrogen and nitrogen, of nitrogen and oxigen, of water and the oximuriatic compounds, in the multiples of oxigen in the nitrous compounds; and those of acids in salts, observed by Drs. Wollaston and Thomson; and above all, in the decompositions by the Voltaic apparatus, where oxigen and hidrogen, oxigen and inflammable bodies, acids and alkalis, &c. must separate in uniform ratios.

water; and the experiments that I have detailed confirm, as well as can be expected with these conclusions.

The proportions of potash and soda indicated, in different neutral combinations, by these estimations, will be found to agree very nearly with those derived from the most accurate analyses, particularly those of Mr. Berthollet; or the differences are such as admit of an easy explanation.

Hyperoximuriate of potash.

I stated in my last communication the probability, that the oxigen in the hyperoximuriate of potash was in triple combination with the metal and oximuriatic gas; the new facts respecting the peroxide confirm this idea. Potassium, perfectly saturated with oxigen, would probably contain six proportions; for, according to Mr. Chenevix's analysis, which is confirmed by one made in the Laboratory of the Royal Institution by Mr. E. Davy, hyperoximuriate of potash must consist of 40·5 potassium, 32·9 oximuriatic gas, and 45 of oxigen.

I have mentioned, that by strongly heating the peroxide of potassium in oximuriatic acid, all the oxigen is expelled, and a mere combination of oximuriatic gas and potassium formed. I thought it possible, that at a low temperature a combination might be effected, and I have reason to believe, that this is the case. I made a peroxide of potassium, by heating potassium with about twice the quantity of nitre, and admitted oximuriatic gas, which was absorbed: some oxigen was expelled on the fusion of the peroxide, but a salt remained, which gave oximuriatic gas, as well as muriatic acid, by the action of sulphuric acid.

Its formation explained.

It seems evident, that in the formation of the hyperoximuriate of potash one quantity of potash is decomposed by the attraction of oximuriatic gas to form muriate of potash; but the oxigen, instead of being set free in the nascent state, enters into combination with another portion of potash, to form a peroxide, and with oximuriatic gas.

The proportions required for these changes may be easily deduced from the data which have been stated in the preceding pages. 5 proportions of potash, equal to 240 grains, must be decomposed, to form with an equal number of proportions of oximuriatic gas, equal to 164·5 grains, 5 proportions of muriate of potash equal to 367 grains; and 5 of oxigen,

oxigen, equal to 37·5 grains, combined with one of potash, equal to 48, must unite in triple union with one of oximuriatic gas equal to 32·9, to form one proportion, equal to 118·4 grains, of hyperoximuriate of potash.

(To be concluded in our next.)

VI.

Farther Account of a Mule Animal between the Male Ass and Female Zebra. In a Letter from THOMAS ANDREW KNIGHT, *Esq., F.R.S., &c.*

To W. NICHOLSON, Esq.

Dear Sir,

IN a former number of your Philosophical Journal* you have given an account of a mule animal between the male ass and female zebra, which was bred by the present Earl of Powis; and you have expressed a wish to obtain farther information respecting it: I in consequence send you the following particulars.

Offspring of an ass and zebra.

You have justly stated, that the zebra would not admit the approach of the ass till his coat had been properly painted to resemble her own; which circumstance is curious, because it goes far to prove, that animals, in a state of nature, distinguish and select those of their own species, in part at least, by sight; while in a state of domestication, when their colours become varied by the influence of cultivation, they appear to be guided almost entirely by another sense.

Wild animals distinguish their species by colour.

The animal, which I proceed to describe, like other mules, bore, externally, a greater resemblance to its male than to its female parent; and until by near approach its stripes, which were much less distinct than those of the zebra, became visible, it was not readily distinguishable from

The animal more resembled the male than the female parent,

* Received from the Right Hon. Sir Joseph Banks, Bart., P. R. S.; and inserted Vol. II, p. 267 of the quarto series.

a very

a very large and strong Spanish ass. I am ignorant whether nature has given to the zebra, as to the ass, the power of breathing through its mouth as well as through its nostrils; or whether the passage of the breath is confined to the nostrils only, as in the horse: but I observed, that the mule zebra uttered its cry, which a good deal resembled the braying of an ass, through its mouth; corresponding in this respect with the mule, which is obtained from the male ass and the mare, and differing from that which is derived from the horse and the female ass.

was intractable.

The temper of the mule zebra, as might have been expected from its parentage, was sullen, vindictive, and untractable. It was nevertheless sufficiently subdued to permit itself to be ridden; but a considerable time generally elapsed before the mule and the rider could agree about the direction in which they were to move; and when that point was in some degree settled, the labour, to the rider, of impelling and guiding his companion, was found so much to exceed that of walking on foot, that the services of the mule were not much in repute, or often called for.

and a complete mule.

Attempts were made to obtain offspring from it both by the female ass, and the mare; but neither were successful. It appeared to possess passions; but, like other mules, to be without powers. It met its death by an accident when rising four years old, and consequently before it had acquired its full growth and strength: but its size and form, at that age, indicated great powers of bearing weight and undergoing fatigue; and it would probably have been of great value both as a beast of burden and draught, had not its temper disqualified it for either office.

Died from an accident at four years old.

I am, dear Sir,

Your obedient servant,

THOMAS ANDREW KNIGHT.

Downton, April the 26th, 1811.

VII.

VII.

Remarks on Potassium, Sodium, &c.; in Reply to the Communications of JUSTUS. *By* JOHN DALTON.

To Mr. NICHOLSON,

SIR,

IN perusing the former of the two communications, purporting to be a reply to the remarks on potassium and sodium in my New System of Chemical Philosophy, (Journal, vol. 28, p. 67) I felt interested in various acute observations of your correspondent; but at p. 72, where he investigates the quantity of oxigen in a given volume of oximuriatic acid gas, I am quite at a loss to conceive how he had obtained so small a portion as 30·24 per cent, when I had found 50, (New System, p. 560) calculating from the best data I could procure, and which I was confident from my own experience could not be materially incorrect. Being at that time particularly engaged, I could not attend to the subject farther than to write a short note (Journal, p. 157) requesting an explanation. This was given in the ensuing number, (p. 219.) When I stated, that his *data* were *defective*, I did not mean *erroneous*; no mathematician would have understood me in that sense; I meant, that he had not given sufficient data, and consequently that he had made the problem an unlimited one. If I should propose the following question to your correspondent, namely, *How long would a body be in moving with a uniform velocity from the Earth to the Moon, or through a space of* 240,000 *miles*—would he not find it necessary, that the velocity should be given? Yet he has found means to answer a similar question without the requisite data. The accuracy of the answer then may well be suspected. It may be of service to your correspondent, and perhaps to others of your readers, if I make out this charge more particularly. According to Chenevix,

Quantity of oxigen in oximuriatic acid.

77·5 mur. acid + 22·5 oxigen = 100 oximur. acid, *by wt.*

then, by measure 77·5 mur. acid $\frac{1·73}{1·125}$ × 22·5 oxi. = 112 measures;

 That

That is, 77·5 measures of muriatic acid + 34·5 measures of oxigen, together 112 measures, will, when chemically combined, be equal to *x* measures of oximuriatic acid gas. How your correspondent ascertains the value of *x* in the above equation to be 100, I know not. It may as easily and as probably be assumed 50 or 500. Surely he is not so ignorant of Mr. Davy's experience as not to know, that 77·5 measures of muriatic acid gas + 34·5 of oxigen, are far inferior in weight to 100 measures of oximuriatic acid. The truth is, the *specific gravity* of oximuriatic acid gas is a *datum* most obviously necessary in the estimation of the oxigen a given volume of it contains.

Potassium contains potash and hidrogen.

With regard to the facts and arguments respecting potassium and sodium, I can bring forward the following, namely, that fused hydrate of potash consists of potash and water, or potash, hidrogen, and oxigen; that in the decomposition of this article by Voltaic electricity, nothing but oxigen gas is evolved, and potassium remains; hence I conclude, that potassium contains, and probably consists of potash and hidrogen. If your correspondent is not satisfied with these facts, and this reasoning, I cannot convince him. The first fact I adopt from my own experience and that of others, the second from that of Mr. Davy; and I am not able to discover any flaw in the conclusive argument.

Heat separates water and potash, and evaporates them at a certain degree, whether chemically combined is not known.

As to the question, what is the power that produces the separation of water and potash? I answer, *heat*. When I say, that, by the application of heat to a certain degree, "the alkali and water both evaporate," no one has authority from me to add "in a state of chemical union," nor yet "in a separate state," though only one of the two ways is likely to be true. The fact was, I had not ascertained when I wrote that, nor indeed have I yet, which of the two is true. I am rather inclined to the latter; but as this is one of a large class of chemical facts, I wish to have more experience, and more time to reflect upon it, than at present I possess. It forms an important inquiry according to my views of chemistry, to ascertain the relation of water to the acids, alkalis, &c. in the very act of distillation; namely, whether the water in passing over is in a state of steam, such as we find it in the atmosphere, or in a gasiform state of

of chemical union with the acid, &c. But, whichever be the case, it is true, "that the process cannot be used to expel the last portion of water from the alkali", when the object is to obtain a ponderable mass of alkali free from water.

When fused potash is exposed to a red heat in an open vessel, white fumes are observed to play over it; these, no doubt, are the particles or small drops of the condensed liquid hydrate, similar to the visible mist or condensed steam over hot water. From this and the above observation, then, it is probable, that in the gun barrel experiment, not only particles or atoms of hydrate of potash, but also of potash, and of steam, may come into contact with the red-hot iron; hence may be explained the production of hydruret of potash or potassium, of oxide of iron, of hidrogen, and of the white amalgam or alloy of potash and iron. This last is easily exhibited by keeping carbonate of potash in fusion for some time in an iron spoon by an intense heat; after the potash is washed off, the whole surface of the spoon, which has been in contact with the fused carbonate, is white as if tinned, and may be acted upon by an acid without losing its colour*.

Potash exposed to heat in an open vessel, and in a gunbarrel.

Alloy of potash and iron.

As for the complex nature of the decomposition of hydrate of potash, I see no great reason to wonder at it. The article consists of 3 elementary principles; so does wood. Why, it may with equal propriety be asked, does wood, in its decomposition by heat, exhibit such a mixture of principles? Charcoal, water, carbonic acid, carbonic oxide, carburetted hidrogen, and hidrogen, are among the products of the destructive distillation of wood.

Complex nature of the decomposition of hydrate of potash.

I was surprised at your correspondent's observations on the argument I have drawn for potassium being a compound of hidrogen from its levity. I venture to say, that Mr. Davy will allow the argument to have some force. In the place referred to, Mr. Davy does not say a word about the notion, *that hidrogen united to potash ought to make a compound specifically lighter than potash.* His answer, which is pertinent and to the purpose, is to those who object to potassium and sodium being classed amongst the metals,

Levity of potassium.

* On potassated iron see also Journal, Vol. XXV, p. 51.

 merely

... on account of their levity. I feel no repugnance to ... those new bodies metals, be they hydrurets or not; but ...ould be far from inferring, that the other metals are ... hydrurets. With respect to the resemblances between the new metals and sulphuretted hidrogen, &c., they certainly are many and striking; so are their resemblances to the metals; I do not undertake to decide which are most numerous. I apprehend a piece of brass or other alloy has as many properties resembling the metals as potassium and sodium; yet no one allows the former to be simple substances. This argument is at least sufficient to show, that simple and compound bodies may have so many points of resemblance, as to be fairly arranged in the same class. I do not consider the discovery of the new metals less valuable and important for being compound rather than simple bodies; and though there may be several facts and experiments, which seem to point them out as simple substances, yet till the preceding facts are controverted, the others can do little more than excite doubts on the subject.

Simple bodies and compounds may have resemblances enough to be classed together.

Your correspondent, adverting to the combustion of potassium in muriatic acid, argues, that the hidrogen is derived from the acid, and not from the potassium; and as a support of the opinion adduces Mr. Davy's experiments, in which a mixture of equal parts of oximuriatic acid and hidrogen is by the electric srk converted into muriatic acid. Granting the truth of the last deduction, the argument amounts to this, that, if two bodies, one of which is known to contain hidrogen, by their mutual action develope that gas, it follows, that the other may not contain hidrogen. But the principal aim of introducing this subject into the discussion on potassium and sodium seems to have been, to defend the notion of oximuriatic acid being a simple substance, and muriatic acid a compound of it and hidrogen. It is to this object that his calculation is directed, on which I have animadverted at the commencement of this letter. The experiments alluded to on oximuriatic acid and hidrogen I consider of the most difficult execution, and if Mr. Davy has succeeded in obtaining tolerable approximations to accuracy in his first trials, great merit is undoubtedly his; still we want a more accurate and

Combustion of potassium in muriatic acid no proof, that it does not contain hidrogen.

The nature of oximuriatic acid not yet ascertained.

and trust-worthy table of the specific gravity of muriatic and oximuriatic acid gasses before the value of the experiment can be duly appreciated; and it should be further ascertained what proportionate condensation of volume is produced upon muriatic acid gas by admitting to it $\frac{1}{3}$ of its weight of water. It is curious to observe, that your correspondent was fully persuaded, and probably continues to be, of the "incontrovertible argument" the above experiments afford to Mr. Davy's opinions in regard to oximuriatic acid, though he did not know at the time he wrote, whether the specific gravity of muriatic acid was 1·4, or 1·9, but took it at 1·7; and he now adopts 1·258; and of the specific gravity of oximuriatic acid he makes no mention whatever; neither of the condensation or contraction of volume which a very small portion of water produces on muriatic acid gas; yet it is impossible to ascertain the bearing of the experiments, till these three data are all of them pretty accurately investigated. If your correspondent wish to institute his calculus anew, I shall give him all the information I can respecting oximuriatic acid: its specific gravity by my own experience is 2·34; by Mr. Davy's, 2·45; by Thenard and Gay-Lussac's, 2·47; and by Dr. Thomson's, (in a letter to me) 2·71. If the last estimate should be true, he will find, that, adopting Mr. Davy's notions and estimate of muriatic acid, there should arise nearly $2\frac{1}{2}$ measures of muriatic acid gas from a mixture of 1 of oximuriatic acid and 1 of hidrogen.

Specific gravity of gasses still a desideratum of importance.

There is one opinion on which we all concur; that it is very desirable the specific gravities of the various gasses should be ascertained within narrower limits. From what is stated above, it appears, we have a range from 1·3 to 1·9 for muriatic, and from 2·3 to 2·7 for oximuriatic acid. Would it not be a proper object for the Royal Society to depute a committee of its members to undertake the investigation? As long as it is left to individuals, each one finds a result differing from that of another; and one authority is deemed as good as another; so that it will, if no such step is taken, be a long time before a general agreement respecting these points is likely to be obtained.

Manchester,
May the 11th, 1811.

I remain, Yours,
JOHN DALTON.

VIII.

VIII.

Table of the Rain, that fell in various Places in the Year 1810, *by the Rev.* J. BLANCHARD, *of Nottingham; with a Meteorological Table for the same Year, by Dr.* CLARKE, *of that Town.*

RAIN TABLE, by the Rev. J. BLANCHARD, of Nottingham.

1810.	Bristol.	Chichester.	London.	Chatsworth, Derbyshire.	Derby.	Horncastle, Lincolnshire.	Ferriby, Kingston upon Hull.	Heath, near Wakefield, Yorkshire.	Manchester.	Lancaster.	Dalton, Lancashire.	Kendal.	Feltfoot, near Milnthorpe, Westmoreland.	Carlisle.	Nottingham.
January - -	*no ac.*	0·28	0·26	0·58	1·10	1·14	0·64	0·89	1·39	2·17	2·85	2·68	4·87	1·84	1·05
February - -	0·90	2·90	1·44	1·15	1·84	1·64	1·10	1·95	2.57	1·91	2·54	4·15	3·11	1·22	1·03
March - - -	2·30	2·84	2·54	2·10	1·50	1·71	0·94	3·45	3·19	2·37	6·03	4·26	8·00	3·80	1·40
April - - -	1·68	1·61	1·70	1·92	1·33	0·82	1·54	1·91	1·92	0·37	1·12	1·03	2·30	1·04	1·00
May - - -	1·42	1·46	1·04	2·89	3·20	2·40	2·66	3·13	1.41	0·12	0·75	0·81	0·60	0·53	2·60
June - - -	2·59	0·49	0·56	0·87	1·42	1·54	1·27	1·90	1·90	1·47	1·87	2·10	1·92	1·60	1·18
July - - -	1·55	4·72	3·78	2·23	3·01	3·50	3·77	4·41	5·50	3·14	3·89	3·40	4·55	3·24	3·85
August - -	4·52	3·07	2·46	2·92	3·40	4·13	4·33	3·18	5·00	3·58	4·18	4·54	4·75	3·22	2·61
September -	2·66	1·95	1·98	2·13	1·85	0·10	0·58	2·10	1·90	2·58	2·62	2·07	2·60	1·70	0·62
October - -	2·66	3·31	1·92	1·73	2·52	2·40	2·21	1·88	4·68	4·00	4·70	3·97	5·43	3·12	2·72
November - -	3·45	11·77	6·08	4·59	6·16	5·23	5·98	5·12	3·68	4·50	5·10	4·01	4·86	3·15	3·02
December - -	6·80	4·53	2·94	4·87	2·26	3·47	3·95	4·30	6·03	6·47	7·19	8·41	8·28	4·30	2·07
Total -	30·53	38·93	26·70	27·98	29·50	28·08	28·97	34·22	39·17	32·68	42·84	41·52	51·27	28.76	23·15

METEOROLOGICAL TABLE,

By Dr. Clarke, of Nottingham.

1810.	Thermom.				Barometer.				Weather.		Winds.			
MONTH.	Maximum.	Minimum.	Medium.	Greatest Variation in 24 hours.	Maximum.	Minimum.	Medium.	Greatest Variation in 24 hours.	Fair.	Wet.	N. and N. E.	E. and S. E.	S. and S. W.	W. and N. W.
January....	53	18	36	10	30·36	29·75	30·05	0·29	26	5	5	9	17	8
February ..	54	14	37	16	30·34	28·73	29·75	0·68	19	9	7	4	21	7
March	59	30	43	10	30·10	28·88	29·62	0·41	19	12	17	9	14	11
April......	70	32	47	9	30·18	29·27	29·76	0·33	24	6	14	7	18	2
May	68	29	47	15	30·33	29·05	29·86	1·05	23	8	26	4	7	6
June	78	38	57	10	30·35	29·72	30·38	0·35	28	2	16	7	15	4
July	77	42	57	15	29·95	29·40	29·75	0·31	12	19	9	4	12	5
August	80	40	57	10	30·43	29·39	29·79	0·52	21	10	1	6	18	6
September..	82	39	56	11	30·38	29·71	30·10	0·31	28	2	17	6	10	5
October....	68	24	45	8	30·30	29·03	29·86	0·54	23	8	15	11	8	6
November..	53	26	38	10	30·12	28·86	29·44	0·55	25	5	11	10	8	7
December..	50	19	36	10	30·50	28·85	29·62	0·71	21	10	5	2	9	21

ANNUAL RESULTS AT NOTTINGHAM.

Thermometer.	Wind.
Highest Observation, Sept. 2d, 82°	E.
Lowest Observation, Feb. 20th, 14°	NE.
Greatest Variation in 24 hours, February 19-20 .... 16°	
Annual Mean............... 46°	

Barometer.	Wind.
Highest Observ. Dec. 31st, 30·50	NE.
Lowest Observ. Feb. 19th, 28·73	SW.
Greatest Variation in 24 hours, May 20th .... 1·05	
Annual Mean .... 29·83	

Weather.	*Days.*
Fair........	269
Wet......	96
	365

Winds.	*Times.*
N. & NE....	143
E. & SE.....	79
S. & SW.....	157
W. & NW. ..	88
	467

Rain.	*Inches.*
Greatest Quantity in July	3·85
Smallest ditto in September ..	0·62
Total Quantity for the Year..	23·15

The Barometer is firmly fixed to a standard wall, on an elevation of 130 feet; and the Pluviameter is placed in a garden, 140 feet from the level of the sea.

IX.

On the Use of Iron Pipes for conveying Water, and Mode of securing their Joints. In a Letter from Mr. JOSEPH T. PRICE.

To W. NICHOLSON, Esq.

Esteemed Friend,

IF the enclosed facts should appear to thee likely to be of any service to those, who may want a supply of water conveyed from any distant source, thou art welcome to give them to the public in thy Journal.

I am,

Thine very sincerely,

Neath Abbey, J. T. PRICE.
14 *Feb.* 1811.

Water-pipe of iron, Mr. H. B. Way, of Bridport, had occasion, in 1805, to lay down between eight and nine hundred feet of iron pipe, in lengths about 6 feet each, and 3 inches in the bore. At every 50 feet was a joint with flanches and screws; the other pieces were put together spigot and faucet fashion. To make these tight, he wrapped round the spigot end some canvas well saturated with white lead mixed with oil to a proper consistence, and drove it into the faucet end as tight as possible. When a length of 120 feet was laid down, the end was plugged up to try the joints; and it was found, that two thirds of them leaked considerably. Being informed by a neighbouring mason, that a linen manufacturer had completely stopped the leaks in his bleaching cisterns, in which lie both cold and boiling was used, by means of Parker and Wyatt's Roman cement, he procured some of this, and luted every joint with it. In 12 or 14 hours the pipe was tried, and found to be perfectly water-tight at the joints; but one of the pipes had a crack in it, which leaked, and this was as effectually stopped by the cement.

leaking at the joints, secured with a luting of Roman cement, which also stopped a crack in it, and in a leaden pipe.

The lead pipe, used in the house, had also a leak in it, which

which was stopped in a very short space of time by the cement.

This work was done between the 10th and 24th of December, in very frosty weather; and the pipe was covered with earth before the end of that month. It is about two or three feet beneath the surface, in a loose, sandy soil; and was kept constantly full of water, without any appearance of leaking.

The water at first discoloured, but this remedied by lime.

The water was so much discoloured by the iron for a week or two, and on standing deposited so much sediment, that it could not be used. To remedy this the same mason recommended to put some unslacked lime into the upper reservoir, or head of water, and open all the cocks below to give it a quick run; which he said would leave a coat of lime round the inside of the pipe, so as to prevent the rust from coming off. This was done, and for a few days after the water tasted very much of the lime; but the taste soon went off, and the water, which is very soft, was as good after it had passed through the pipe as at its source.

Another water pipe with a nearly constant stream soon cleared itself.

In the following autumn the same gentleman superintended the laying down of 360 feet of similar iron pipe, with a fall between 20 and 30 feet, the joints of which were secured in the same way. The supply of water here was so copious, that it was obliged to be kept running all night and great part of the day. This soon cleared the pipe from rust, so that after a few days the water came through colourless, and consequently no lime was used.

Two more laid down.

In the summer of 1808, or 1809, two more pipes were laid down in the neighbourhood in a similar manner, extending together between two and three thousand feet; and with equal success.

All continue sound.

These were all perfectly sound and secure in the month of February last; a little before which Mr. Way, having occasion to put a new leaden pipe in his yard from the iron one, found the latter, as far as it was examined, apparently as good as when laid down, and the cement as perfect, only seeming harder.

X.

X.

On the Invention of the Economical Process for Evaporation ascribed to Montgolfier. *In a Letter from Mr.* St. Amand.

To W. NICHOLSON, Esq.

SIR,

Economical process for evaporation said to be invented by Montgolfier.

THE supplement to the XXVIIIth vol. of your interesting and excellent Journal of Natural Philosophy, &c., just published, contains an account of a process, abridged from vol. LXXVI of the Annales de Chimie published at Paris in November last. This process *for procuring and accelerating the evaporation of fluids, without employing heat produced by the ignition of combustible substances*, is said to have been communicated in conversation by Mr. de Montgolfier. This declaration of Messrs. Desormes and Clément, authors of the article in the Ann. de Chim., is not translated in your Journal, which gives only an abridgment of it: but you may easily turn to it, and I beg you will have the goodness to satisfy yourself of the fact. I am neither jealous nor envious of the fame of any one; on the contrary I deem myself happy in having fallen on the same idea and the same means with a man of deserved celebrity: but truth and justice give me a right to claim at least a priority of date in the invention, and to this I shall confine myself. The following are incontestable proofs of it.

But Mr. St. Amand has a prior claim.

Proofs of this.

After the disastrous and bloody catastrophe of the 10th of August, 1792, I took refuge in England, whither I brought with me the same process, which I employed myself in developing and varying in several ways; giving it a greater extent, and applications more numerous, than those mentioned in the Ann. de Chim., or in your Journal. These developements were proposed and submitted to the British government about fifteen years ago. They were known, approved, and patronized by several persons, distinguished for

for their rank, knowledge, and situations; by ministers, peers, members of parliament, &c. Several learned societies, artists, and government contractors, whom the minister was desirous of exciting to carry it into execution, were acquainted with it. It is now nine years since the manuscripts, which contained a full account of the invention, with various other matters, and which were in the hands and under the care of government, were taken away by some treachery, respecting which there are only conjectures.

Papers stolen from a government office.

About that time, sir, I had the honour to request you to assist me with your knowledge and distinguished talents, in rendering them into the English language, with which I have but an imperfect acquaintance; and to show you authentic certificates of the experiments, that I had made several years before with an apparatus, to which I gave the name of a *polychrest machine*, on account of the variety and multiplicity of its applications. I appeal to your candour and impartiality, to confirm the proposal I had the honour to make you on this subject, and the production of the certificates, which are still in my possession, if you still remember the circumstance; which indeed was the occasion of my first having the honour of being known to you.

Farther proofs.

Polychrest machine.

The apparatus I have mentioned, for which I obtained several *caveats* in the patent office more than twelve years ago, was ordered by the nobleman who was then first Lord of the Admiralty, whose kindness and encouragement have supported me, and whose protection I have still the honour to enjoy. His zeal for the good of the public, and for the sciences, induced him to cause the apparatus to be constructed at his own expense, under my direction, as appears by the certificate of the experiments made in his presence, signed by himself some time after, and dated in 1798, which I have in my possession. After such authentic official testimonies, I presume I need not appeal to several others, which, though highly respectable in themselves, would add nothing to the validity of the proofs already adduced. All these persons, of whom I could give you a list, are still living; and as most of them are known to you, they would confirm, if requisite, the publicity, which I beg the favour

Caveats at the patent office.

An apparatus constructed here and tried in 1798.

favour of you to give this letter in the next number of your Journal.

I have the honour to be,

With a just and high admiration of your talents,

Your very humble and very obedient servant,

ST. AMAND.

No. 25, York Buildings, New Road,
May the 15th, 1811.

XI.

On the Combustion of Ether, and of Metals, in Oximuriatic Gas: by Mr. VAN MEERTEN, *and Mr.* STRATINGH *.

Combustion of different substances in oximuriatic gas.

AS a proof of the property of sulphuric ether to burn with flame in oximuriatic acid gas, leaving a little oxide of carbon, Mr. Van Meerten points out the following experiment.

Ether.

Let a piece of the whitest possible sulphate of lime remain some time in ether. Set fire to this piece well soaked in ether, and introduce it under a jar filled with oximuriatic gas: the ether, or rather its hidrogen, will burn rapidly, and the surface of the gypsum will be covered with a coat of oxide of carbon.

Brass.

The combustion of brass and of tin is effected in this gas as easily as that of iron in oxigen. Take a slender brass wire, twisted into a spiral, and terminated by a piece of kindled charcoal; immerse it in a jar of oximuriatic gas; and it will burn rapidly and entirely, throwing out sparks. At the same time it may be seen, that the charcoal has not the property of burning in it, for it remains unaltered. A tin wire exhibited the same phenomena.

Tin.

* Ann. de Chimie, vol. LXXIII, p. 87. Translated from Trommsdorff's Journal der Pharmacie, by Mr. Vogel.

A copper

A copper wire does not burn in this gas, but becomes as soft as lead. Copper.

A brass wire not heated redhot does the same. Brass.

This gas has no action on lead-wire. Lead.

A wire of red French gold melted, without throwing out sparks. Gold.

Pure silver wire, and iron wire, were not altered in it. Silver.

Mr. Stratingh, in verifying the preceding experiments, prefers making the extremity of the brass wire red hot, to adding a burning coal to it. He could not succeed in burning tin wire. Brass. Tin.

He effected the combustion of a very slender copper wire, the extremity of which was pointed and red hot. The inside of the jar was covered with green oxide of copper. Copper.

A wire of ducat gold did not grow red, or melt, in the gas, but was slightly oxided. This difference probably arose from Mr. Van Meerten's French gold containing more copper. Gold.

Very slender silver wire melted, after its extremity had been made red hot. Silver.

Iron wire by itself was not altered: but on adding to its extremity a wire composed of an alloy of three parts of antimony with one of tin, which was heated a little before its immersion, the iron wire gave out much red vapour, and the inside of the jar was covered with a beautiful red oxide of iron. Iron.

Camphor alone does not burn in this gas: but if a piece be stuck in the end of a cleft stick, wrapped round with tin foil, and this powdered with metallic antimony, the camphor will begin to burn with a deep red flame. Camphor.

Oil of turpentine, or of cloves, poured into this gas, gives out some fumes, but very little light*. Essential oils.

* A rag wetted with oil of turpentine takes fire in oximuriatic gas. *Vogel.*

XII.

XII.

Observations in Illustration of Mr. HOWARD'S *Theory of Rain. In a Letter from* THOMAS FORSTER, *Esq.*

To W. NICHOLSON, Esq.

SIR,

AS the following observations may serve farther to illustrate Mr. Howard's ingenious Theory of Rain, (see his paper on the modification of clouds,) I shall request your insertion of them in your scientific Journal.

Appearance of clouds on the 18th,

On the 18th inst. the day was close and warm, in the afternoon I observed several different modifications of cloud dispersed about in the atmosphere at different altitudes. In some places *cirro-stratus* might be distinguished; in others, the clouds shewed a tendency to *cirro-cumulative* aggregation, *cumuli* increased in density, and cirrose fibres transversely crossed their summits, forming *cumulo-stratus*, which like mountains transfixed by the mighty shafts of giants appeared in the horizon, and represented a majestic appearance; while in other places the process of *nimbification* appeared going on rapidly, and distant thunder was heard. About six o'clock the sky, seen between the clouds under the descending sun, appeared of a very unusual brownish lake colour. As the evening advanced the mountainous clouds in the horizon appeared of a deep blackish blue colour, their edges as well as those of other detached clouds above them exhibiting a bright golden colour. Flocks of *cumulus* floated along in the wind, and refracted dark lake coloured light; by degrees all the clouds lost their distinctive characters as separate modifications, and became one dense mass, which ended in rain during the night.

19th,

On the 19th it rained all the morning, but held up in the evening; the continuous sheet of cloud however remained, notwithstanding a strong wind from the north.

and 20th of May.

Early on the morning of the 20th the same uniform sheet of cloud obscured the sky. As the day advanced it broke, and this dense sheet of *nimbus*, which had been originally formed by the collapse of several distinct modifi-

cations

cations, appeared to resolve itself into them again; as the sheet broke part of it seemed to mount up into a higher and comparatively calm region, and formed itself into *cirro-cumulus*, in some places disposed like windrows of hay, in others consisting of small roundish nubeculæ of various sizes; and into *cirro-stratus*, consisting either of flat sheets of thin vapour with dentated edges, or disposed in streaks: other parts of the once continuous sheet of *nimbus* descended into a lower region, and floated along in flocks of *cumulus*, with a strong wind, and the day became very fine. In the evening the distinct modifications again seemed lost in a general mistiness of the atmosphere, which as it became darker seemed very red coloured, and this vapour was seen to thicken in particular places which became dense *nimbi* again, and gave forth vivid flashes of lightning, and thunder storms continued through the night.

From the evident decrease in the quantity of cloud during the fine part of the day, it is evident, that, while part of the sheet of *nimbus*, which obscured the sky in the morning, divided itself again into the several modifications, the collision of which originally formed it; great part must have been absorbed by the air*: this is farther probable from the great transparency and dense blue colour of the sky between the clouds.

The insertion of these observations in your next number will, if convenient, oblige your constant reader,

Clapton, Hackney, THOMAS FORSTER.
May 21, 1811.

XIII.

Observations on Dr. BOSTOCK'S *Review of the Atomic Principles of Chemistry. By* JOHN DALTON.

To Mr. NICHOLSON.

SIR,

DR. Bostock's dissertation on the atomic system of chemistry in your Journal (Vol. XXVIII, page 280) may be divided Dr. Bostock's remarks on the

* See Mr. Van Mons's paper in your Journal for September, 1809; also Rees's Cyclopedia, article *Cloud*.

[illegible] into two parts; one of which relates principally to the theory of chemical combinations, which I have embraced from an extensive comparison of facts and observations furnished by the writings of others, and from a careful and laborious train of experimental investigation of my own; the other relates to *his* application of the theory to the solution of a few of the more simple and common combinations. On the former of these parts I beg leave to make a few observations; on the latter I think it is altogether unnecessary to say any thing, unless it be to correct a misrepresentation or two which Dr. Bostock has accidentally introduced, namely, that oxigen and [illegible]gen in water are as 85·7 to 14·3 in weight, and that [illegible] umbers are as 7 to 1 nearly (page 285); and that I conce[illegible]e *nitrous oxide* to be a *binary* compound (page 290). [illegible] weights of oxigen and hidrogen in water are stated [illegible]. New Syst. p. 275) to be 87·4 and 12·6 re[illegible] d it is *nitrous gas* which I maintain to be [illegible]nd nitrous oxide and nitric acid to be *ternary* con[illegible]

Meaning of the terms, theory and hypothesis.

I do n[illegible]Dr. Bostock as to his remarks on the [illegible]eory and *hypothesis*; these terms as far as I can learn from their common use differ only in degree. Theory is all or the greatest part of the facts reduced to regular laws; *hypothesis* is where only a few facts are reduced to laws, and the rest are either irreducible, or are yet only in a train, or have their accuracy suspected. I think no one would seriously advance any hypothesis on any subject, that had not some one or more facts previously established in its favour. What is *theory* to one man may be *hypothesis* to another. If Newton had lived in an age when no mathematician but himself existed, he might have established his beautiful theory of gravitation to his own satisfaction; but it must have been only an *hypothesis* to the rest of his contemporaries. These observations lead me to remark farther, that my chemical doctrine on combination is not, "altogether hypothetical," according to Dr. Bostock's own definition. I remember the strong impression which at a very early period of these inquiries was made by observing the proportion of oxigen to azote, as 1, 2 and 3, in nitrous oxide,

oxide, nitrous gas, and nitric acid, according to the experiments of Davy. And Dr. B. must confess, that the greater part of the facts I have stated in my book, as the grounds from which I draw my conclusions, are not new; but facts that have been investigated by others before me, and often with the same results.

Dr. Bostock must be aware, that in writing my System of Chemistry, I have presumed all along, that the future readers of it would be tolerably acquainted with the various branches of the mechanical philosophy; otherwise I must have made a cyclopedia of it; one department must have treated of statics, another of dynamics, another of hydrodynamics, another of pneumatics, &c. This was not my design. If therefore I have announced certain rules as proper to be laid down, and have given no demonstration of them, it was because they were deemed obvious to the class of readers I expected, or otherwise were such as could not be demonstrated but by the gradual developement of the system itself in its progress.

Mechanical consistency of Mr. Dalton's first rule of combination.

I proceed now to point out the mechanical consistency of the 1st rule, which Dr. Bostock has quoted, page 283, namely, that "when only one combination of two bodies can be obtained, it must be presumed to be a *binary* one, unless some cause appear to the contrary"; and if this be established, the other three which he quotes may be considered as corollaries from it.

Instance in the composition of water.

Let us suppose a mixture, for instance, of hidrogenous and oxigenous gas, in such sort, that there are the same number of atoms of each gas: now as the gasses are uniformly diffused, each atom of hidrogen must have one of oxigen more immediately in its vicinity. The atoms of hidrogen are all repulsive of each other; so are those of oxigen: the atoms of hidrogen are all equally attractive of those of oxigen, and the attraction increases in some unknown ratio as the distance diminishes. Heat, or some other power prevents the union of the two elements, till by an electric spark, or some other stimulus, the equilibrium is disturbed, when the power of affinity is enabled to overcome the obstacles to its efficiency, and a chemical union of the elementary particles of hidrogen and oxigen ensues.

 Now

Now the question is, whether, according to the received laws of motion, each one atom of oxigen should unite to the one of hidrogen next to it, or whether 7 atoms of oxigen should leap over all the more proximate atoms of hidrogen to another at a greater distance, and consequently less attractive, and that finally only $\frac{1}{7}$th of the number of atoms of hidrogen should be engaged by the oxigen and the rest remain in a state of freedom as before. The former is the conclusion I adopted, and thought it would scarcely require any elucidation; the latter is thought by Dr. Bostock equally plausible as the former (page 291). However till it can be shown, that a [illegible] force [illegible] ercome a greater, I must refuse my assent. [illegible] si [illegible] is another consideration, that has no sm [illegible] t with [illegible] it is known, that the oxigen [illegible] ies th [illegible] art [illegible] at, and in all probability of its [illegible] ion [illegible] h it in its combined state; it would [illegible] phen [illegible] on, if it could be rendered visi[illegible] le, to see 7 ato[illegible] o[illegible] gen surrounding 1 of hidrogen of equal size, [illegible] atoms of oxigen repelling one another, but retain[illegible] an atom of hidrogen at the centre, whilst a number of atoms of hidrogen are around, all equally attractive of oxigen with the one engaged. But the difficulty does not end here: though I am persuaded the relative weights of the hidrogen and oxigen in water are nearly as 1 to 7, I by no means assert, that they are accurately so. Perhaps Dr. Bostock would prefer the ratio of 15 to 85; that is, in the smallest integers, 3 to 17. Now upon this view of the subject we must picture to ourselves 3 atoms of hidrogen surrounded by 17 of oxigen as constituting 1 atom of water; the remaining 14 atoms of hidrogen must be conceived to continue in their elastic state as spectators, and not to disturb the equilibrium of the atom of water. This *may* be the constitution of an atom of water; but it is wonderful, that in the decomposition of it by galvanism, nothing but hidrogen and oxigen should be produced, and never any new combination should arise out of so complex a system of particles as an atom of water exhibits to view. Would it not have been an improvement to have formed a set of atoms on purpose for water, by melting 3 of hidrogen into one, and 17 of oxigen into one?

Both you and your readers will probably think by this time,

time, that I have proceeded far enough in the developement of the truth of a proposition almost self evident; if not I may resume it on some future occasion.

The 2d, 3d, and 4th rules are necessarily consequent to the 1st. When an element A has an affinity for another B, I see no mechanical reason why it should not take as many atoms of B as are presented to it, and can possibly come into contact with it (which may probably be 12 in general), *except so far as the repulsion of the atoms of B among themselves are more than a match for the attraction of an atom of* A. Now this repulsion begins with 2 atoms of B to one of A, in which case the 2 atoms of B are diametrically opposed; it increases with 3 atoms of B to 1 of A, in which case the atoms of B are only 120° asunder; with 4 atoms of B it is still greater as the distance is then only 90°; and so on in proportion to the number of atoms*. It is evident then from these positions, that, as far as powers of attraction and repulsion are concerned, (and we know of no other in chemistry) *binary* compounds must first be formed in the ordinary course of things, then *ternary*, and so on, till the repulsion of the atoms of B (or A, whichever happens to be on the surface of the other), refuse to admit any more. 2d, 3d, and 4th rules.

I shall now proceed to the 5th, 6th and 7th, or remaining rules, which Dr. Bostock has not quoted, but of which he is equally in want of an explanation, or he would not have formed such conjectures as that "*it seems* the most natural to regard the sulphuric acid as the *binary* compound of sulphur and oxigen," and that carbonic acid is a *binary* and carbonic oxide a *ternary* compound, and that nitric acid is a *binary* compound of azote and oxigen and nitrous gas a ternary, and that nitrous oxide is *binary*, &c. (page 5th, 6th, and 7th rules.

* I find from the principles of statics, that, upon the supposition of spherical atoms of equal size, and that the law of repulsion after chemical union is the same as before, namely, reciprocally as the central distance, the repulsion of any one atom of B upon another of B, to separate it from A, is a constant quantity, on whatever point of the surface of A it may be placed; so that when there are 3 atoms of B, the 3d atom is repelled twice as much by the other two as it would be by a single atom placed diametrically opposite. When there are 4 atoms, then the 4th is three times as much repelled, &c.

 287,

5th, 6th, and 7th rules.

287, 290. The 5th rule is "that a binary compound should always be specifically heavier than the mere mixture of its two ingredients." The principle on which this rule is founded is recognised by chemists as *general*, if not *universal*; namely, that condensation of volume is a necessary consequence of the expulsion of heat by the exertion of affinity. Thus, steam is specifically heavier than a mixture of 2 parts hidrogen and 1 oxigen; ammoniacal gas is in like manner heavier than 21 azote with 72 hidrogen. The 6th rule is that "a ternary compound should be specifically heavier than the mixture of a binary and a simple, which would, if combined, constitute it; and the 7th, that "the above rules and observations equally apply when two bodies, such as C and D, D and E, &c. are combined." These rules are founded on the same principle as the former, which principle entirely precludes the notions of nitrous oxide and nitric acid being binary compounds, and discountenances those of carbonic and sulphuric acid being binary compounds.

After making these observations on the general rules, I shall now advert to more particular objects. I have already remarked, that explanations and elucidations similar to the above were what I thought unnecessary to enter upon in the work alluded to: it is not improbable but I may have been mistaken in this respect, especially if such inquiries and observations as the following should be frequent. "When "bodies unite only in one proportion, whence do we learn "that the combination must be binary? Why is it not as "probable, that water is formed of two atoms of oxigen "and one of hidrogen, of two atoms of hidrogen and one "of oxigen, or in short of *any assignable number of atoms* "*of hidrogen and oxigen?* I do not perceive that Mr. "Dalton has given any reason in support of this binary "combination in preference to all the rest; and *I am una-* "*ble to conjecture what reason can be urged in its favour*," (page 283). I hope such remarks will be no more adduced; and farther, that if any one should inquire, for instance, why 1 part of carbone, which takes 1·28 of oxigen, or 2·56, does not also occasionally take 3·84 and 5·12 parts of oxigen, it will be understood, that the reason I should assign is,

 that

that in the state of carbonic acid there are two atoms of oxigen combined with one of carbon, and a third or fourth atom of oxigen, however it may be attracted by the carbon, cannot join it, without expelling one or more of the atoms of oxigen already in conjunction. The attraction of the carbon is able to restrain the mutual repulsion of two atoms of oxigen, but not of three or more.

The drift of Dr. Bostock's remarks and objections, in page 285, is quite beyond my comprehension. The *single object* I had in view in writing the paragraphs there quoted was, to find the relative weights of hidrogen and oxigen in a pound or any other given weight of water. I have deduced them as 1 to 7; whether right or wrong may be a question: but certainly I had no *other* object in view, and therefore I consider *that* as the only one to which any criticism can properly apply.

Looseness of quotation.

I must object to such loose quotations as the following; namely, that I have assumed, "that when only one combination of two elementary bodies can be obtained, it must be binary;" my language is, "it must be *presumed to be* a binary one *unless some cause appear to the contrary.*" Supposing for instance, that my hypothesis had been formed previously to the discovery of carbonic oxide, I must have concluded, according to Dr. Bostock's quotation, that carbonic acid was a binary compound; whereas I should have compared carbonic acid with the other acids, and found that like them it ought to contain at least two atoms of oxigen to one of base, and this with me would have appeared "some cause to the contrary." Again, "only one combination of oxigen and hidrogen, and only one combination of hidrogen and azote can exist," (page 284.) Knowing that I never entertained such ideas, I was curious to find out those passages in my book, which could possibly be so far misapprehended, and I think they must have been the following: "As only *one* compound of oxigen and hidrogen is certainly known," (page 275), and "only one compound of hidrogen and azote has yet been discovered," (page 415). These ideas however are repeatedly ascribed to me, and in the most express manner. "We have never yet

t been able to produce more than one combination with each of these substances, therefore Mr. Dalton concludes, that only one combination can possibly exist," (page 286, s h a note.)

Size of atoms not dependent on their weight.

ugh I am fully persuaded we are in possession of data sufficient to decide upon the relative *weight* of atoms, we are not in regard to their *size*. This last is a matter of mere speculation. Dr. Bostock seems to think the *size* must be in direct proportion to the *weight*. I should however rather suppose, that atoms of different bodies may be made of matter of different *densities*, if the expression may be allowed; thus mercury, the atom of which weighs almost 170 times as much as that of hidrogen, I should conjecture was larger, but by no means in the proportion of the weights, which would require a diameter of five or six times the magnitude. Perhaps in a question of this sort Newton has a better claim to be heard than either of us; he says, (I think in the 31st query to his Optics) " God is able to create particles of matter of several *sizes* and *figures*, and in several proportions to the space they occupy, and perhaps of different *densities* and forces······at least I see nothing of contradiction in all this."

Knowing that Dr. Bostock had occasionally communicated several chemical essays through your Journal, I was curious to see whether he had not furnished me with some arguments in behalf of that doctrine, which he thinks " depends for its proof entirely upon *subsequent* observations and experiments." In the XIth vol. of this Journal, page 75, May 1805, he has given valuable analyses of the acetate and superacetate of lead. The results give the proportions of lead and acid as under:

Superacetate—Lead	6·12	or	100
Acid	3	··	49
Acetate—Lead	8·4	or	100
Acid	2	··	24

A number of such analyses as these would compel Dr. Bostock, and others of your chemical readers, to examine the theory of chemical combinations which I have offered to them

them with more attention, than I fear they do. The present state of chemical science imperiously demands it.

I remain, yours &c.

Manchester, JOHN DALTON.
May the 15th, 1811.

SCIENTIFIC NEWS.

Royal Society of Edinburgh.

ON the 4th of March, Mr. Allan read a paper on the rocks of the environs of Edinburgh, being the first of a series, which he proposes to read on this subject. The present embraced the rocks of St. Leonard's Hill and Salisbury Craig. The specimens illustrating the subject he presented to the Society, to be deposited in their cabinet. Rocks in the environs of Edinburgh.

On the 18th, Sir George Mackenzie read some geological remarks on the appearance presented by different rocks in Iceland; and showed their importance in connecting the phenomena of volcanoes with the principles of the Huttonian theory. Sir George brought forward the results of Sir James Hall's experiments on heat modified by compression, and successfully applied them to support his conclusions. The facts were explained in a satisfactory manner, and the whole paper was so important in a geological point of view, that we regret that it is not in our power to give an analysis of it. We understand, however, that it will form a part of the account of Iceland, which Sir George and his friends are about to publish, the work is now in the press. Huttonian theory. Account of Iceland.

On the 1st of April, Dr. Brewster read a description of a new instrument, for measuring capillary attraction, the instrument to be exhibited at a future meeting. Capillary attraction.

Prof. Playfair read a very interesting paper, being part of his new edition of his illustrations of the Huttonian theory, entitled Remarks on the natural History of Volcanoes, Professor Playfair's illustration of the Huttonian theory.

Royal

Royal Medical Society of Edinburgh.

The Society will give a set of books, or a medal of five guineas value, to the author of the the best experimental essay in answer to the following question.

Prize question. Does any decomposition of acids and alkalis take place on their uniting to form neutral salts, according to an opinion lately advanced by Mr. Davy in respect to muriates?

Honorary, extraordinary, and ordinary members of the Society are alone invited as candidates. The dissertations are to be written in English, Latin, or French, and to be delivered to the Secretary on or before the first Day of December, 1812. And the adjudication of the prize will take place in the last week of February following. To each dissertation is to be prefixed a motto; and this motto is to be written on the outside of a sealed packet, containing the name and address of the author. No dissertation will be received with the author's name affixed; and all dissertations, except the successful one, will be returned, if desired, with the sealed packet unopened.

Wernerian Natural History Society.

At the meeting on the 6th of April, Mr. William Elford Leach laid before this Society an arrangement of the natural tribe of diptera, eproboscidea of Latreille, with descriptions of the species, which he illustrated by drawings and specimens. At the same meeting Prof. Jameson read an account of the occurrence of coal in the first sandstone formation in Thuringia and Silesia; whence he inferred the possibility of coal existing in the extensive depositions of red sandstone in Scotland, in which that valuable mineral has not hitherto been discovered.

Diptera.

Coal in the first sandstone.

Society's Memoirs. The first volume of the Wernerian Society of Natural History has just been published.

India Maritime Directory. The 2d part of the Maritime Directory for navigating to, from, and between the Ports of India, China, &c., by James Horsburgh, Esq., F. R. S. is in the press, and is expected to be ready for publication in July.

Report

Report of the Proceedings of the Mathematical and Physical Class of the French Institute, continued from p. 79.

Mr. Vauquelin has analysed tobacco, with a view to detect the principles, that characterise this plant, and have occasioned it to be chosen for the uses for which it is employed: and to ascertain the changes produced in it by the preparations it undergoes for sale. It appears to contain an animal matter of the albuminous kind, malate of lime with excess of acid, acetic acid, nitrate and muriate of potash, a red matter the nature of which is unknown, muriate of ammonia, and finally an acrid and volatile principle apparently different from any other known in the vegetable kingdom. It is this principle, that imparts to tobacco its well known qualities; and it may be extracted from the plant by distillation, and employed separately. Prepared tobacco yielded, in addition to the matter above enumerated, carbonate of ammonia, and muriate of lime. Analysis of tobacco. Peculiar principle in it.

Mr. Vauquelin imagined, that the juice of belladonna, from its effects on the animal economy being analogous to those of tobacco, might contain the acrid principle he had discovered in the latter: but on analysing it he found only an animal matter, salts with base of potash, and a bitter substance, from which the juice of belladonna receives its narcotic properties. Analysis of belladonna.

Mr. Chevreul presented to the class a very extensive series of experiments on vegetable matters. Te object o f some of these was the bitter principle produced by the action of nitric acid on organic matter containing nitrogen. He conceives it to be a compound of nitric acid and an oily or resinous vegetable matter: and he ascribes its detonating property to the decomposition of the nitric acid, the formation of ammoniacal gas, prussic acid, olefiant gas, &c. But with the amere is produced a resinous matter, and a volatile acid, on which Mr. C. has made many experiments; and which he considers as differing from the amere only by a small addition of nitric acid. Production of amere

Another object of Mr. Chevreul was the substances formed by the action of nitric acid on carbonaceous or resinous matters, which have the property of precipitating and of artificial tanuin.

gelatine

gelatine. Mr. C. does not agree with Mr. Hatchet, their discoverer, in considering them as similar to tannin. He thinks they differ not only from tannin, but from each other; and that their differences arise from the acid employed, the matter from which they are prepared, and the quantity of acid that enters into their decomposition.

Sulph. acid and camphor.

Mr. C. has likewise examined the different compounds formed by the action of sulphuric acid on camphor.

Distillation of spirits.

Not a year passes without presenting us with some happy application of chemistry to the arts, and thus affording us fresh proofs of the benefit, that our manufactories derive from the sciences. Thus Mr. Chaptal has made some interesting observations on the distillation of wine. The improvement of this process has gone hand in hand with that of chemistry. One of the principal distilleries in the South of France is nothing more than Woulfe's apparatus on a large scale.

Ancient colours.

The same gentleman has analysed seven specimens of colours found at Pompeia.

Stucco, &c.

Mr. Sage has examined the processes best adapted to the management of lime for making solid mortars; the nature of different kinds of stucco; the means of giving the polish of marble to artificial stones; and lastly a process for reducing white wax to a soap.

Zinc for roofs.

He has also written a paper, and Messrs. Guyton and Vauquelin a report, on the advantages and disadvantages of roofing houses with zinc.

Injurious manufactories.

The section of chemistry have also pointed out, at the desire of the minister, what manufactures may be injurious to those who live in the neighbourhood; and what measures should be adopted, to reconcile the interests of the manufactures with those of the public.

Indelible writing ink.

A report has also been made on a paper of Mr. Tarry's respecting the composition and improvement of writing ink. The author has composed an ink, which is not destructible either by acids or alkalis; a great advantage in France, where the practice of altering title deeds has lately been very prevalent. It has the inconvenience however of letting fall its colouring matter too easily.

Artificial turquoises,

Another report, on the artificial turquoises of Mr. Sauviac, gives

gives reason to hope, that the productions of art in this respect will soon imitate exactly those of nature, so as to afford us a new source of wealth.

A committee has also examined the late Mr. Bachelier' composition of a preservative mortar. Preservative plaster.

The progress of mineralogy has not been great. Mr. Guyton however has made known a new crystalline form of the diamond, and has made some valuable experiments on the tenacity of metals. New diamond crystal.

From the researches of Mr. Sage it would appear, that the chrysolite of volcanoes, when powdered, may be substituted for emery. All the artists that have used it have expressed themselves satisfied with it. Substitute for emery.

The observations from which geology can draw the most important conclusions are no doubt those relating to fossil animals, particularly such as have lived on the earth. Mr. Cuvier has continued his inquiries into this subject. Jointly with Mr. Brongniart he has concluded his mineralogical geography of the environs of Paris; and he has since examined the bony brecciæ of the coasts of the Mediterranean. These singular rocks, which are found at Gibraltar, near Terruel in Arragon, at Cette, at Antibes, at Nice, in Corsica, on the coasts of Dalmatia, and in the island of Cerigo, have been formed in fissures of compact limestone, which constitues the principal part of these countries, and are all composed of the same elements; which are numerous fragments of bones, and of the surrounding limestone, confusedly united together by a brick-coloured cement. All the bones belong to herbivorous animals, most of them known, and even still living in these places. These are mingled with freshwater shells; which lead to the supposition, that the brecciæ are posterior to the last abode of the sea on our continents, though very ancient with respect to us; since we have no indication of such brecciæ being formed in the present day, and some of them, as those of Corsica, include unknown animals. Fossil animals. Bony brecciæ.

Bones of animals of the order glires are contained also in allnvial soils. They have been found in the bogs of the valley of la Somme, with horns of stags, and heads of oxen; and in the vicinity of Azof, near the Black Sea. These bones belonged to animals of the genus castor; some much resembling those of the common beaver; others, which form Bones in alluvial soils.

— a complete head, from a larger species. Mr. Fischer, who discovered this animal, called it trogontherium, which Mr. Cuvier has adopted for its specific name.

Bones in schist.

Remains of glires have been found also in schists. Three species have been described, and Mr. Cuvier has seen the figure of one, which some authors consider as having belonged to a guineapig, others to a polecat. He could not determine the genus however, though it has the characters of the order glires.

Fossil bones of a species of elk.

Among the fossil bones of ruminants found in the loose soil, Mr. Cuvier has recognised a species of elk, different from that now known. Its remains have been collected in Ireland, in England, on the banks of the Rhine, and in the vicinity of Paris, in beds of marl of little depth, which appear to have been deposited in fresh water. Other horns, discovered in abundance, near Etampes, in sand underlying fresh water limestone, prove the existence of a small species of reindeer not now known. Mr. C. has also observed remains of the horns of the roebuck, fallow-deer, and stag, which do not appear to differ from those of the known species. None are more abundant than these.

Skulls of the aurochs, urus?

Among the fossil remains of ruminants with hollow horns he has recognised skulls of the aurochs, found on the banks of the Rhine and the Vistula, in the neighbourhood of Cracow, in Holland, and in North America. These skulls differ only in size from those of the present aurochs, and this Mr. C. ascribes to their more abundant nourishment in the vast forests and fat pastures of Germany and Gaul.

and of the original of our domestic cattle,

Fossil skulls are also found, that differ from those of our domestic cattle only in being larger, and having a different direction of the horns. These have been dug up in the valley of la Somme, in Suabia, in Prussia, in England, and in Italy. "If we call to mind," says Mr. C. "that the ancients distinguished two sorts of wild oxen in Gaul and Germany, the urus and the bison, shall we not be tempted to suppose, that one of them was that, which, after having furnished our domestic breed of cattle, has become extinct in the savage state; while the other, incapable of being tamed, still subsists in very small numbers only in the forests of Lithuania?"

the bison?

Bones of the horse and boar.

We find likewise in the loose soil bones of the horse and the

the boar. The former almost always accompany the fossil elephants, and are found with the mastodontes, tigers, hyenas, and other fossil bones, discovered in alluvial lands: but it has not been possible to determine, whether they belonged to a species different from our domestic horse. Those of the latter have been obtained chiefly from bogs, and exhibit no mark to distinguish them from those of the common boar.

New species of manatee.

Other bones have been found, which belonged, according to Mr. Cuvier, to a new species of manatee. They were in strata of a course marine limestone, on the banks of the Layon, near Angers; and were mingled with other bones, some of which appeared to have belonged to a large species of seal, others to a dolphin.

Three singular reptiles.

The skeletons of three fossil oviparous quadrupeds, found in calcareous schist, have likewise been examined by Mr. Cuvier. One was at Oeningen, on the right bank of the Rhine, at its efflux from the lake of Constance. It had been described and figured as the skeleton of an antediluvian man, an errour already refuted. Mr. C. has shown, that it was a reptile, somewhat resembling the salamanders, and belonging to the genus proteus.

The second, found at the same place, was of the toad genus, and approaching to the bufo calamita.

The third, and the most singular, discovered in the quarries of Altmuhl, near Eichstadt and Pappenheim in Franconia, had been described and figured by Colini in the memoirs of the Academy of Manheim. Mr. Cuvier considers it as having belonged to a species of *saurien.* The length of its neck and head, its long mouth armed with sharp teeth, and its long arms, indicate that it fed on insects which it caught flying; and the size of its orbits leads to the supposition, that it had very large eyes, and was a nocturnal animal. No reptile now known bears any resemblance to this inhabitant of the ancient world.

Ornitholite.

In a supplement to his fossils of Montmartre, Mr. Cuvier has given a figure and description of an ornitholite much more perfect than any before published. It appears to have been of the gallinaceous order, and to have come nearest in size to our common quail.

Mr.

Petrifi... , Sage has described some carpolites. One was a ker... o... a walnut, found at Lons-le-Saulnier; another ap-pear... to have been the fruit of the wild nutmeg, that grows at Madagascar and in some of the Molucca islands; and the th rd belonged apparently to a genus approaching the durio. The last was converted into jasper, the other two into li nestone. To these observations Mr. S. adds some others, that had been made before, and concludes from them, that the petrified fruits found in our climates are exotic. He likewise enters into a chemical investigation of the means by which these petrifactions have been effected.

New order of plants. Ord ... ...nod will always be two objects of the greate... ...ortance in natural history, and particularly in botany, ... accordingly the most celebrated naturalists have ma... ...m their p... ...lar study. Mr. de Jussieu, who ma... ...e cons... ...s the legislator of methods in bota... ...formed a ... order of plants under the name o... ...era, of which he composes it, are ... ...and perhaps citrosma, pavo-nia, ...rmia. ...s order should be placed im-mediately ... ...e family of utriceæ: but at the end of the monimiæ Mr. de J. places the calycanthus, hitherto united with the rosaceæ, which he considers as the type of a new order, that will serve as an intermediate link between the monimiæ and utriceæ.

Fructification of grasses. Mr. Palissot-Beauvois has studied the organs of fructifi-cations in grasses more accurately than had been before done; and on the structure of each part of these organs has founded characters, that distinguish them from each other; thus affording means of arranging the numerous species in genera much more natural than those hitherto adopted.

New plant of the palm kind. Mr. Labillardiere has made known a new plant of the family of palms, of which he makes a genus under the name of ptychosperma, bordering on the elates and arecas. This plant was discovered by the author in New Ireland. It frequently reaches the height of sixty feet, and yet its trunk is but two or three inches in diameter. From these proportions Mr. L. gives it the specific name of gracilis. It is astonishing, as he observes, that so slender a tree should support

support itself: but we know, that all the monocotyledonous plants have the hardest part of their wood externally; and this structure imparts to them a degree of strength, which they could not possess if their most solid fibres were in the centre.

Mr. Lamouroux has presented to the class a very extensive work on marine plants. In forming one group of all these Mr. L. has made a useful innovation. The little progress that has been made in the study of seaweeds has prevented botanists from being agreed respecting the organs of fructification. Mr. L. not only embraces the opinion of the male and female organs being placed in tubercles at the extremity of their ramifications, but characterises the different parts of these organs with precision. He has farther observed, that the species growing on granite, on limestone, and on sand, are always different. As to their interior organization, Mr. Decandolle had observed, that it was destitute of vessels, and entirely formed of cellular texture. Mr. L. distinguishes two sorts of cells; one very long, and hexagonal, forming the stalks, and the ribs of the ramifications; the other also hexagons, but nearly equal sided, and constituting the membranous or foliaceous substance. The former he supposes are analogous to the vessels, and the latter to the cellular texture of the more perfect vegetables. His researches have also led him to form several new genera. Marine plants.

To CORRESPONDENTS.

On perusing Dr. Davy's paper in our present number, and the letter from Mr. J. Davy, in which he mentions the properties of Dr. Davy's zuthic acid, or compound of oximuriatic gas and oxigen, p. 43 of our last number, J. M. will probably perceive, that the objects of his obliging communication are there answered.

To some other correspondents a similar remark will apply.

METEOROLOGICAL JOURNAL,

For MAY, 1811,

Kept by ROBERT BANCKS, Mathematical Instrument Maker, in the STRAND, LONDON.

Day of APR.	THERMOMETER.				BAROMETER, 9 A. M.	RAIN, noted at 9 A. M.	WEATHER.	
	9 A. M.	9 P. M.	Highest in the Day	Lowest in the Night.			Day.	Night.
28	56°	55°	63°	47	29·60		Fair	Cloudy
29	53	50	55	46	·37	·080	Rain	Ditto
30	51·5	50·5	55·5	47	·77	·195	Ditto	Rain
MAY								
1	56	57	60	52	29·67	·055	Ditto	Ditto
2	56·5	52	59	47·5	·64	·140	Ditto	Fair
3	54	56	58	52	30·02	·040	Ditto	Ditto
4	55·5	57	61·5	53	29·89	·190	Ditto	Cloudy
5	56	51	56	44	·72	·200	Cloudy*	Ditto
6	48	50	52	47	30·07		Rain	Ditto
7	53	52	53	46	29·75	·320	Ditto	Ditto
8	49	54	56	48	·81	·160	Cloudy	Rain
9	52	51	54	47	·56	·110	Rain	Ditto
10	55	56	60	50·5	·74	·325	Ditto	Fair
11	56	59	62	55	·83	·070	Fair	Ditto†
12	59	68	72	60	·74		Ditto	Ditto
13	65	66	73	62	·53		Ditto	Ditto
14	65	57	65	52	·50		Ditto	Ditto
15	58	60	68	56	·78		Ditto	Rain
16	60	60	67	57	·79	·025	Rain ‡	Fair
17	58	61	65	55	·90	·010	Fair	Ditto
18	60	64	67	58	·86	·030	Ditto	Cloudy
19	60	55	62	50	·95	·140	Rain	Ditto
20	55	57	70	54	·85	·260	Fair	Cloudy §
21	57	60	62·5	55	·68	·200	Cloudy	Fair
22	60	63	67	52	·70		Fair	Rain ‖
23	57	57	64	52·5	·81	·300	Ditto	Fair
24	58	62	66	57	·91		Fair	Cloudy ¶
25	61	64	69·5	58	·99	·090	Ditto	Ditto
26	65	68	74	62	30·04		Ditto	Ditto**

2·940 Inch. since last Journ.

* Boisterous day. † Lightning. ‡ Thunder, 3 P. M.
§ Lightning—Tremendous Thunder and Lightning at 4 A. M. ‖ Ditto at 8 P. M.
¶ Lightning. ** Ditto.

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

JULY, 1811.

ARTICLE I.

On the Manufacturing of Thread, and Articles resembling Flax, Hemp, Tow, and Cotton, from the Fibres of the common Nettle. By Mr. EDWARD SMITH, *of Brentwood, Essex**.

SIR,

I HAVE the honour to transmit to you a short memoir on that hitherto much neglected and despised vegetable the nettle, with the general useful purposes to which the produce thereof may be applied. If you think it will merit any claim to the attention of the Society, I request you will do me the favour to lay it before them. Uses of the nettle.

My attention was first directed to this matter about the year 1793, but from many impediments no favourable opportunity presented itself for particular investigations till about the year 1800, since which time, I have annually selected a few of the nettle plants from their various situations at different periods, in order to ascertain the state most

* Trans. of the Soc. of Arts, vol. XXVIII, p. 109. The silver medal was voted to Mr. Smith.

congenial to the process, and that most suitable to the different purposes to which I thought them applicable. The result of my experiments has deeply impressed upon my mind, that they may be made subservient to national utility, particularly at the present period, when our foreign commerce is so generally impeded, and in consequence our supplies of foreign hemp and flax nearly annihilated.

Its abundance.

I beg leave to observe, that the growth of nettles is general, in every country, particularly in strong fertile soils, that on every bank, ditch, and place, which cannot be brought to tillage, they are produced in such abundance, that the quantity, if collected, would be of great magnitude.

Places adapted to its growth.

The growth of them might be encouraged in such waste places, or a vast quantity of land of that description might, at a moderate expense, be made to produce a valuable crop of a useful article heretofore regarded as a nuisance. The shady places in woods, parks, and coppices, are particularly favourable to their growth; I have found them in such situations in the greatest perfection in point of length and fibre. The harl, or fibre of them, is very similar to that of hemp or flax, inclining to either according to the soil and different situations in which they grow. I have ascertained, as far as I have been able to proceed, that they may be substituted for every purpose for which hemp or flax is used, from cloth of the finest texture down to the coarsest quality, such as sailcloth, sacking, &c., and for cordage.

Answers every purpose of hemp or flax.

Material for paper.

Another very material use, the magnitude of which, I trust, will be duly estimated, is, that they may be applied to the manufactory of paper of various qualities. The impediments to foreign commerce have lately deprived us of a supply of linen rags, and occasioned a general use of cotton rags in the paper manufactory, which is injurious to the preservation of the most valuable works in literature, to the truth of which the observation of every one must bear testimony, who has attended to the depreciated quality of writing and printing papers.

That the produce of nettles, and the refuse of them from the manufactory, may easily be converted into writing, printing, and all inferior sorts of paper, I feel confidently assured.

assured. For the purpose of writing and printing paper they might be gathered twice in one season, as for these uses the length of staple is not required, and the fibre would be considerably increased in its fineness; and in point of colour, either in the refuse or unwrought state, the chemical process of bleaching now in practice would render them a delicate white.

I have in possession some samples, which have gone through a succession of processes similar to what are practised on hemp and flax; and I have, without the aid of any implements, brought them to a state of preparation ready for the hackle; but for want of that, and there being no flax or hemp manufactory in this neighbourhood, I have not been able to proceed farther, but I judge that they are sufficiently advanced so as amply to evince the practicability above referred to.

If you think proper, I will transmit the samples for the Society's inspection, and give any farther information in my power.

Permit me the honour to subscribe myself, Sir,

Your most humble servant,

March 24, 1809. EDWARD SMITH.

SIR,

I am much obliged to the Society for their reference of my communication to one of their Committees. About ten years subsequent to my first observations, and three to my first experiments, I observed the following paragraph in the Chelmsford Chronicle, November 25, 1803. "The Society of Œconomy, at Haërlem, has offered prizes for the best memoir as to the particular species, the season for gathering, and the manipulation necessary in preparing nettles for use." This is the only account I have ever seen of them, and shows that such a matter was regarded as deserving the attention of that Society; but as I from the first had it in contemplation to present my observations on the subject to the Society of Arts &c., and thinking the matter of great consequence, and wishing my own country to be benefited by it, I declined answering the Haërlem advertisement.

Prize on the subject offered by the Society at Haërlem.

My discovery of the properties of the nettle is original, and arose entirely from my own observations on the apparent resemblance to hemp and flax, which I remarked they had when growing. I now transmit to you some samples, in different states, for the Society's inspection.

I have the honour to be, with great respect,

Your most humble servant,

March 28, 1809. EDWARD SMITH.

SIR,

Coarse yarn & flax from nettles.

I have now the honour to transmit to the Society my farther progress, viz. A sample of yarn prepared from the coarsest part of the nettle produce, which I deemed less liable to be injured for want of knowledge in the manufacturing than the finer qualities. Since my former letters I have been bleaching some of the nettle flax, and have brought it to so good a colour, that a preparation from it would produce paper perfectly white, and I have caused a sample of yarn to be made from the nettle produce, both of which I have sent.

Paper from them.

I likewise enclose an improved specimen of paper made from the same substance; also a preparation for paper, a part of the same sample the enclosed was made from, which is, of course, much inferior to what would be done by a paper manufacturer. These samples having been made by such rough instruments as were constructed by my own hands, and which of course the Society will consider.

I remain respectfully, Sir,

Your obliged humble servant,

Nov. 18, 1809. EDWARD SMITH.

The following Specimens, produced from Nettles by Mr. Smith, are deposited in the Housekeeper's Office.

Articles produced from nettles.

Samples of the fibres, in their rough state, resembling different kinds of hemp and flax.

Samples of the fibres equal to the finest flax, and remarkably strong in texture.

Samples of very strong yarn, prepared from the coarsest fibres.

Samples

Samples of coarse paper, prepared from the rough refuse fibres.

Samples of the coarse fibres bleached white.

Samples of a coarse substance resembling cotton prepared from the bleached coarse fibres.

Samples of white paper prepared by him from the last-mentioned substance.

Mr. Smith's *Process for preparing various Articles from Nettles.*

The kind of nettle capable of being manufactured into cloth, &c., it is scarcely necessary to say, is that which in general is denominated the stinging nettle. The most valuable sort, which many years practical experience has furnished me with a knowledge of, in regard to length, suppleness, fineness of the lint, brittleness of the reed, which dresses most freely, with less waste of fibre, and yields the greatest produce of long and fine strong harl, I have found growing in the bottom of ditches among briars, and in shaded valleys, where the soil has been a blue clay or strong loam, but from which situations I have selected some which have measured more than twelve feet in height, and upwards of two inches in circumference. Plants growing in the situations above described are in general from five to nine feet in height, and those growing in patches on a good soil, standing thick, and in a favourable aspect, will average in height about five feet and a half, will work kindly, and the stems are thickly clothed with lint. Those that grow in poorer soils, and in less favourable situations, with rough and woody stems, and have many lateral branches, run much to seed, are stubborn, and work less kindly; they produce lint more coarse, harsh, and thin. In every situation and different soil I have experienced the most productive nettles to be those which have the smoothest and most concave tubes, the largest joints, the fewest leaves, and which produce the least quantity of seed.

The kind of nettle. *Best sort.* *Worst nettles.* *Marks of the best.*

In gathering them, as they are perennial plants, I have preferred the mode of cutting them down, instead of pulling them up by the roots. This I recommend to be the practice, with a view to obtain a second crop where the situations will

They should be cut.

will allow of it, and to secure the propagation of them the subsequent year.

Time of gathering.

The most favourable time for collecting them is from the beginning of July to the end of August, but it may be continued even to the end of October, only the lint of those which remain growing to that time will be less supple, and will not work so freely; and if the season happens to be unfavourable, it is probable there would not be sufficient time to steep and grass them, in which case they should be dried by the heat of the atmosphere, or if the state of the weather would not permit of this, then by means of artificial heat; and when dried they should be housed or stacked till the spring, when they might successfully undergo the same operation of steeping as those of the first collection. Such as grow in grass fields, where the grass is intended for hay, should be cut when the hay is cut, in order to prevent their being spoiled by the cattle when feeding, the harls of which would be fine in quality, and well suited to be wrought up with the second crop, and which crop may be obtained after those of the first cutting, where the situation will admit of their being preserved. The fine quality of such I ascertained last autumn, and found the height of them to average three feet and a half; they were gathered the latter end of November. The following are the processes adopted by me.

Treatment after gathering.

After the nettles are gathered they should be exposed to the atmosphere till they gain some firmness, in order to prevent the skin from being damaged in the operations of dressing off the leaves, the lateral branches, and seeds. This should be done a handful at a time; and afterward they should be sorted, viz. those which are both long and fine by themselves, those which are both long and coarse by themselves, and those which are short and coarse by themselves; then made up into bundles as large as can be grasped with both hands, a convenient size for putting them into the water, and taking them out; a place for this purpose being previously prepared, either a pond or a pit free from mud, or a brook or river. The bundles should then be immersed, and placed aslant with the root end uppermost, and to prevent their floating on the surface some weight should be laid upon them.

The

Steeping. The time required for steeping them is from five to eight days; but it is better they should remain rather too long in the water than too short a time, yet great care should be taken that they are not overdone. When the fibre approaches to a pulp, and will easily separate from the reed, and the reed becomes brittle and assumes a white appearance, this operation is finished.

Grassing. The bundles should then be taken out singly, very carefully, to avoid damaging the fibres, and be rinsed as they are taken out of the water to cleanse them from the filth they may have contracted; they must then be strewed very thin upon the grass, and be gently handled. When the surface of them is become sufficiently dry, and the harl has obtained a degree of firmness, they should be turned repeatedly, till they are sufficiently grassed; the time required is known only by experience, so much depends on the state of the weather during the process; when they are sufficiently done, the harl blisters, and the stems become brittle; they must then be taken up and made into bundles, and secured from the weather.

Separation of the harl. The harl is now to be separated from the reed, after the manner practised on flax and hemp, either by manual labour or machinery now in use in those manufactories. This operation was performed in my experiments by hand, and with implements constructed by myself, but which I consider too simple here to describe.

Dressing. The harl being separated from the reed, it requires next to be beaten, that it may become more ductile for the operation of dressing, which may be performed with such implements as are used for dressing flax or hemp.

Spinning. This operation being accomplished, the produce of the nettles is arrived at a state ready for spinning, and may be spun into various qualities of yarn, either by hand, or by machinery constructed for the purposes of spinning flax or hemp; and this yarn may be successfully substituted for the manufacturing every sort of cloth, cordage, rope, &c., which is usually made from hemp or flax, and is particularly calculated for making twine for fishing-nets equal to the Dutch twine imported for that purpose, the fibres of the Twine for fishing nets.

nettles

nettles being stronger than those of flax, and not so harsh as the fibres of hemp.

Refuse.

In the course of my experiments on nettles it often occurred to me, that the refuse, and such parts as were damaged in the different processes, with the under-growth, might be applied to useful purposes, and in addition to the nettle manufactory, as applicable to the purposes for which hemp and flax are used. Another source of productive labour of great magnitude would be derived from a new substance, capable of being converted into so many beneficial uses, if my speculations should be finally accomplished. In contemplating these subjects, I was induced to believe the refuse and under-growth might be converted into paper of various sorts, according to the changes they might be made to undergo from the several operations necessary to reduce them to a proper state for this use; having frequently observed, with regret, the deterioration in the quality of writing and printing paper, occasioned by the use of cotton rags in the paper manufactory; which evinces itself even to the most superficial observer, who may only casually open many of the modern publications, and which must be admitted is of the utmost moment, as it endangers the preservation of works of literature. Being convinced of the superior strength of nettle substance, I thought, could my speculations be reduced successfully to practice, it would not only remedy this great evil, and operate as an antidote to the use of cotton rags in that part of the paper manufactory, but eventually effect a reduction in the prices of books, which for some years have been rapidly increasing, and are now become excessive, to the great obstruction of disseminating useful knowledge among mankind, and contribute to the diminution of our exports in that material branch of commerce.

Paper made from it.

Advantages of this.

In addition to the above incentives, the consideration of the high price of paper, chiefly occasioned, as I conclude, from the extravagant price of linen rags, and the impediments to the procuring a foreign supply of them, arising from the circumstances of the times; and seeing that the use of linen cloth is in a great measure superseded by the very general introduction

Farther motives.

introduction of cloth manufactured from cotton, which consequently must materially diminish the supply of linen rags, and probably, in process of time, from the increasing substitution of cotton cloth for linen, linen rags, particularly of the finer qualities, may be totally annihilated. Urged by all these considerations, which were forcibly impressed on my mind, and feeling assured of the practicability of reducing the substance of nettles to a state necessary to the production of paper, and confident in the superior strength of such paper, if it could be manufactured from a substance so substantial, I was most powerfully impelled to attempt to reduce to practice what in theory I had so warmly cherished. The attempt was arduous, not only from an entire want of knowledge of the manufactory, and of the necessary utensils, but I was destitute of any proper implement to engage in the undertaking with any probability of success; hoping however by perseverance to succeed, I proceeded, and found on my first rough trial my expectations realized.

Preparation of the lint for paper.

The most favourable condition of the lint, with a view to the paper manufactory, is to begin with it after it is hackled; in order that the fibres may be divested of the skins which enclose them, as, when it is intended to make white paper, having gone through that process, it would greatly facilitate the bleaching, and be the more easily disencumbered of the gross particles.

When I signify as my opinion, that the fibres of nettles should be dressed the same as for yarn, previous to their being prepared with a view to the making of paper, I wish not to be understood to convey the idea that the operation cannot be dispensed with; because I conceive, that, by the aid of such machinery as is in use with the paper manufacturers, or by some improvements therein, they might be brought to a pulp easily, even when the nettles are first gathered, should it, with a view to saving of labour, be deemed necessary; but the practicability of this I leave to the experience which time may hereafter afford.

Bleaching.

My operation of bleaching the fibres for paper was performed on the grass, which I deem preferable to the new mode of bleaching with water impregnated with air by means

means of oxigenated muriatic acid gas; because the old mode of bleaching on grass weakens the strength of the fibre, leaves it more flexible, and thereby expedites the maceration, which in some degree compensates for the time it requires longer than by the chemical process. But for bleaching of yarn or cloth made of whatever substance the chemical process, if scientifically conducted, experience has convinced me is preeminently superior, as it gives additional strength to the yarn, greater firmnes to the texture of the cloth, and is an immense saving of time, labour, &c.

Subsequent management.

After the lint is bleached it should be reduced to a proper length for paper, and then macerated in water after the manner of rags, and undergo similar processes till the substance is converted into paper, which may be easily accomplished by manufacturers, and the substance of nettles made to produce paper of the first quality and the most substantial.

Mode in which specimens of paper were produced.

In my process the lint was reduced by scissors to particles as minute as was practicable with such an implement; then it was macerated in cold water about ten days, and brought as much to a pulp as could be effected without the aid of grinding, &c. Being a stranger to the composition used to procure the adhesion of the particles, if any is used for this purpose, I tried several glutinous substances, none of which answered so well as a solution of gum, but I am well aware this cannot be generally used, being too expensive.

After the pulp was impregnated with the solution, I then spread it thin on a wire frame of my own construction, which process, except drying it, with me was final. Not having possessed of the means of pressing the paper any more than grinding of the lint, and for want of the film which adheres to the lint being dressed off, I could not completely destroy the colour, so as to produce a clear white without picking out every discoloured particle, which I so well accomplished, that when I had reduced the staple in length, in this state it was perfectly free from colour; the deterioration which ensued when converted into paper was occasioned by the solution of gum.

My processes were the fruits of my own conceptions, and I desire it may not be understood, that I presume to recommend

mend them for practice, being conscious, that the manufacturers of paper, hemp, and flax, from analogy, are possessed of the knowledge of operations and means more consonant and infinitely superior.

These several manufactures from the new substance of nettles, patronized by the stimulating approbation and recommendation of the Society of Arts, &c., I with all due deference venture to predict will rapidly increase the capital of those individuals who engage therein, afford new employment to the poorer classes of society, and become a new source of wealth to the nation.

EDWARD SMITH.

April, 28, 1810,

II.

Description of an improved Reapinghook for Corn. By Mr. JOSEPH HUTTON, Jun. *of Ridgway, near Sheffield**.

SIR,

AT a time like the present, when all foreign supplies of grain are cut off, nothing can be more acceptable to the public than useful discoveries and improvements in agriculture. I am therefore anxious to contribute, in some degree, to this end, by sending some remarks on reaping the harvest, accompanied with my new-improved reaphook. Agricultural improvements important.

I have, for the last eight years, had an opportunity of inspecting the different modes of reaping the harvest in many parts of Great Britain, and I have also had information on the subject from various parts of Europe and America on respectable authorities. Reaping.

I will first endeavour to describe the different kinds of implements used for this purpose, some of them being employed in one part of the kingdom, and not in another.

* Trans. of the Society of Arts, vol. XXVIII, p. 54. The silver medal was voted to Mr. Hutton for this improvement.

The

Sickle. The sickle is of the greatest antiquity, though its use is now much upon the decline in England. It is almost in the form of a half circle, from twenty to thirty inches long, about three fourths of an inch broad, with teeth cut in the edge from twenty to thirty in an inch, inclining from the handle to the point.

Sithe. The sithe is an instrument so generally known, as to need no description, farther than that some are made longer, and others broader, as necessity or caprice requires.

Reapinghook. The common reap-hook is a half-circular piece of iron and steel, from twenty to thirty inches long, about one inch and a half broad, and has a smooth even edge, like that of a sithe.

Badginghook. The badging or baging-hook is broader than the common reap-hook, particularly at the point, where it is most used, and straighter than the sickle or reap-hooks generally are.

Use of the sickle. The reaping of wheat with the sickle is yet continued in Yorkshire, Durham, Westmoreland, Cumberland, Lancashire, Warwickshire, Leicestershire, Northamptonshire, Rutland, Nottinghamshire, and part of Lincolnshire; it is performed by putting the sickle into the corn with the right hand, meeting it with the left hand, gathering the corn into the elbow of the sickle near the right hand, holding the corn fast with the left hand until it is cut, then the person repeats his cutting until he has obtained a large handful, which is generally one third of a sheaf, which he lays in the straw binding ready prepared.

Use of the reapinghook. The common reap-hook is used in the manner above specified, but its effects are far different, the sickle, having a toothed edge, does not cut such stems as are not immediately collected into the left hand; for it is impossible to collect all where dispatch is required, particularly in thin struggling crops, for the teeth of the sickle being inclined, it is not so sharp in cutting from point to handle, as from handle to point, which is evident from a feel with the finger. The reap-hook, having a smooth even edge, cuts both ways alike, and cuts the straggled stems before they are collected in the gathering hand, consequently the loss of grain is great. The hook is allowed to perform its work with more ease

ease than the sickle, which perhaps accounts for its now being so general, nothing else being used for cutting wheat in the following counties:—Cornwall, Devon, Dorset, Somerset, Monmouth, South-Wales, Hereford, Wilts, Hants, Berkshire, and part of the adjoining counties; it is also much used in Norfolk and Lincolnshire, also Northumberland, Westmoreland, and South of Scotland. It has lately been introduced into the North of Ireland by Irishmen who have laboured in Scotland, likewise into the Indies for cutting rice.

The badging-hook is used about London, and in the West of England, its work is performed by the man holding the hook in his right hand, and while, with the left, he reclines the stems intended to be cut upon the standing corn, which supports it when cut, he repeats his cutting from his right to his left hand, and collects it from his left to his right, which is almost a sheaf. Use of the badginghook.

Badging is an expeditious mode of reaping; the corn is cut very low as if mown, and answers where straw is valuable. It may be said, that badging produces more manure, from the greater quantity of straw collected; but in stiff clay lands a longer stubble is perhaps necessary to be left, to render the land lighter for the following crop. The badging-hook is also used for cutting oats in Lincolnshire and Staffordshire, and where labourers can be procured, is preferable to the sithe, being expeditious in its work, and less loss attending its use, the corn is gathered in straight regular order, which is not the case with the sithe; for the sithe requires at least two persons to follow it to bind the corn in sheaves, besides raking the stubble. The corn after the sithe lies in very irregular order, and holds more moisture in wet weather; besides, the sithe is destructive to the ripe corn, for its heavy stroke strips from the entangled stems the best and ripest grain. This preferable to the sithe.

The labourer seldom considers the interest of his employer, but generally uses such a tool as will do the work with most ease to himself.

I offer my improved reap-hook to the public, with a view to prevent the loss of grain, and at the same time to be used with ease by the labourer. It has a smooth edge like the reap-hook Improved reapinghook.

reap-hook, from the handle of it towards its middle, where the corn is gathered; the other part has a toothed edge, like a sickle, and it will not scatter the corn so much as either of the other implements.

I shall furnish certificates to show, that I am the inventor of it, and that it has considerable advantages in general use, It is a great preserver of corn, in harvest, where it is straggled much from heavy rains.

I am, Sir,
Your obedient Servant,
JOSEPH HUTTON, Jun.

The following certificates were received.

Testimonies of its utility.

A certificate from Mr. J. Turner, of Ridgway, dated October 3, 1809, stating, that in the year 1805 he had made two dozen of improved reap-hooks by Mr. Hutton's instructions; that they were the first he ever knew to be made upon this plan, and that in the present year he and others have made thirty-five dozen for him.

A certificate from William Taylor, of Summit Lodge, in Yorkshire, bailiff to G. F. Burton, Esq., dated September 29, 1809, stating, that after a few seasons experience, he finds Mr. Hutton's reap-hook preferable to any other, from the nature of its edge; that the labourers under his superintendance used all of this sort the last season, and that it is found to be a great saver of corn.

A certificate from John Boothe, sithe, sickle, and reap-hook manufacturer, Ford Mills, near Sheffield, dated October 12, 1809, stating, that Mr. Hutton's reap-hook is certainly superior to the common one, and that public opinion confirms it as such, for there has been a great demand for them the last two harvests.

A certificate from Mr. Edmund Littlewood, of Dent Hall, near Dronfield, dated October 15, 1809, stating, that Mr. Hutton's reap-hook is superior to the common ones now in use, especially in the last harvest, in which the crops have been remarkably straggled, and bad to reap, owing to the heavy rains and winds. That the common reap-hooks cut

cut while putting in, before the gathering hand has collected the stems together, and consequently many drop and are lost; which is not the case with Mr. Hutton's new-invented reap-hook, which does not cut before the stems are collected together in the gathering hand.

III.

Report of Messrs. DE PRONY, CHARLES, MONTGOLFIER, *and* CARNOT, *to the French Institute, on the Invention of a new Engine, by Mr.* CAGNIARD-LATOUR, *formerly Pupil at the Polytechnic School**.

IT is known, that all bodies immersed in a fluid lose a part of their weight equal to that of the fluid they displace. On this principle Mr. Cagniard's new engine is founded. Principle of a new engine.

The first mover in this engine is not the vapour of boiling water, as in common steam engines, but a volume of air, which, being conveyed cold to the bottom of a vessel full of hot water, is there dilated; and, by the effort it then makes to rise to the surface, acts in the manner of a weight, but in a vertical direction, agreeably to the principle mentioned above. First mover.

This mover, once discovered, may be employed in different ways: the following is that of Mr. Cagniard.

His machine, properly speaking, is composed of two others, which have perfectly distinct functions. The object of the first is to convey to the bottom of the vessel of hot water the volume of cold air necessary. That of the second, to apply the effort, which this air, once dilated by heat, makes to reach the upper surface of the fluid, to the effect required to be produced. The machine composed of two, answering different purposes.

For the first purpose Mr. Cagniard employs the screw of Archimedes. If such a screw cause a fluid to ascend by turning it in one direction; it is obvious, that it will cause it to descend, if turned in the contrary direction. If then it be The first the screw of Archimedes.

* Journal des Mines, vol. XXVI, p. 465.

be immersed in water, so that only the upper part of its spiral remains in the air, it ought, when turned in the contrary direction, to cause to descend to the bottom of this water the air that it takes into its upper part in every turn. This is precisely what Mr. Cagniard's machine does. The air he wants is first conveyed to the bottom of the reservoir of cold water, in which the screw is immersed; and thence it is conveyed by a pipe to the bottom of the vessel of hot water. The heat of this water immediately dilates it, and thus creates the new power, which is to act as the first mover. In this way the first object of the machine is accomplished.

which conveys air to the bottom of a reservoir of water,

whence it rises into the inverted buckets of a bucket wheel in hot water,

The second, as we have said, is to apply this new mover to the effect to be produced. For this purpose the author employs a bucket wheel completely immersed in the vessel of hot water. The air, dilated and collected at the bottom of the vessel, finds a passage contrived so as to guide it under those buckets, which have their mouths downward. The ascensional force drives the water out of these buckets, and the side of the wheel on which they are being thus rendered lighter, the wheel turns continually like a common bucket wheel.

the motion of which is applied to the purpose wanted.

This wheel, being set in motion, is capable of transmitting its action to any other movable machinery, either by a toothed wheel and pinion, or any other means. In Mr. Cagniard's machine the effect produced consists in raising, by means of a cord fixed to the axis of the wheel, a weight of fifteen pounds, with a uniform velocity of an inch in a second, while the moving power applied to the screw is only equal to three pounds with the same velocity. The effect of the heat therefore is to quintuple the natural effect of the moving power.

The effect of the first mover quintupled.

It may be conceived, that, the moving power being quintupled, we may take from this effect a sufficient momentum to supply the original power, and still there will remain at our disposal four times the original power. This in fact is done in Mr. Cagniard's machine. By means of a crank, he forms a communication between the axis of the wheel and that of the screw, so that this turns as if it were moved by an external agent, and consumes by its motion a fifth of the

Part of this effect taken to supply the place of the power that first sets it in motion.

the momentum of the moving power. The remainder serves to raise a weight of twelve pounds with a uniform velocity of an inch in a second: that is to say, the machine continually winds up itself, and leaves a disposable power, equal to four times what would be necessary in an external agent to keep the machine in motion.

It follows from what has been said, that, in the machine of Mr. Cagniard, the heat at least quintuples the volume of air employed in it; since it is evident, that the effect produced must be proportional to the volume of this air dilated. I have said *at least*, on account of the friction to be overcome: but this friction is very trifling, because both the screw and the wheel, being immersed in water, lose a considerable portion of their weight, and consequently press very little on their pivots. Besides, the movements are slow, and not alternative, and there is no jerk in them; so that this machine is free from those resistances, that commonly consume great part of the moving power in others, and accelerate their wear.

The friction very trifling, and wear but small.

We do not look upon the machine of Mr. Cagniard as an object of curiosity merely: it may be useful under various circumstances. As it produces its effect in a body of water heated only to 75° [167°F.], or even less, it affords an opportunity of turning to account the hot water, that in various manufactories is thrown away, or runs to waste. In saltworks, for instance, the ebullition of the saline solution might be made, by means of Mr. Cagniard's machine, to work the pumps for filling the boilers: in ironworks the heat of the furnace might be made to work the bellows: in common steam-engines, which, like that at Chaillot, furnish a large quantity of very hot water, an action might be obtained equivalent to that of several men, or horses: in fine, in baths, distilleries, potteries, limekilns, glasshouses, founderies, and wherever there is a production of hot water, or of heat, advantage might be made of Mr. Cagniard's machine. This machine, which, as has been said, is liable to very little friction or want of repair, has also the advantage of being easily managed; and when its action is suspended for a time without extinguishing the fire, the heat is not lost;

It may be useful in practice in various situations.

lost: for, as the water is not boiling, the heat accumulates in it, and furnishes afterward a more powerful action.

The screw of Archimedes may be used in this way for blowing large fires with great advantage.

The screw of Archimedes, employed in this machine, produces the effect of a pair of bellows, and might be used as such in a foundery. It may even be considered as the best that is known, not only from its simplicity, solidity, and constant action, but from the saving of power in its use compared with any other machine employed for the purpose; for the screw becomes very light and very movable by its immersion in water, so that the friction of its pivots is next to nothing.

The machine applicable to raising water by means of mercury.

Mr. Cagniard has likewise applied the action of this machine to a body of mercury. As its mechanism requires two fluids of unequal densities, he has merely substituted mercury for water, and water for air, retaining the same construction as is mentioned above. The result is a very simple hydraulic machine, which, without valve, stoppage, or action of fire, being set in motion by any external agent, as a man or a stream of water, gives a continual flow of water at a height fourteen times as great as that of the column of mercury, in which the screw is immersed. This height may even be increased at pleasure, without altering that of the mercury, by combining the action of three fluids, mercury, water, and air. For this purpose, instead of raising a column of water alone a lighter column is formed by a mixture of water and air. This mixture is effected of itself, by disposing the lower part of the pipe that contains this column so as to leave its opening partly in water, partly in air, according as we would have more of one fluid than of the other, and consequently occasion the rise of the mixture to a greater or less height. It is obvious however, that this does not alter the momentum of the moving power, but that, when we would raise the water to a greater height, the machine yields a proportionally smaller quantity. This effect is analogous to that of the Seville pump.

General report.

The machine of Mr. Cagniard appears to us to include many new and ingenious ideas. Its application has been guided by sound theory and a thorough knowledge of the true laws of physics. It appears to us, that it may be useful to the arts on various occasions. We think therefore, that

that the author merits the encouragement of the class, and propose, that its approbation should be given to the machine.

IV.

Description of an Instrument for facilitating the Reduction of Plans; by Mr. DE LA CHABEAUSSIERE*.

I HAVE thought of an instrument for reducing plans, which is so simple, that I am surprised it was not invented by others long ago: but this simplicity, which I consider as an advantage, is probably the reason. I call it a *minudometer*, as its principal object is the reduction of plans; though it will answer equally well for enlarging them. Simple instrument for reducing plans.

This instrument is a wooden rule, with fiducial edges, at the extremity of which is a pivot; or a plate of metal with a hole, into which a pivot may be inserted at pleasure. This pivot is a piece of a needle, with a knob for a head. The minudometer described.

On this rule are marked two scales, one smaller than the other in any proportion you please. As my purpose in making it was chiefly for plans of mines, I took as a basis a scale of 3 lines to a fathom, the proportion generally used for such plans; and for the reduction I employed a scale of one line to a fathom. Such a scale diminishing the length and breadth of a plan two thirds each, all the parts will be brought sufficiently near to be considered at one view. Such a plan may be inferior in minuteness of detail and accuracy to a larger, but it has the advantage of being more portable, and will enable the manager to have a clear idea of the works under his direction. Particularly adapted to plans of mines.

Suppose then I would reduce a plan of three lines to the fathom to a third of this in all its dimensions. I take a rule of two feet long, which appears to me the most suitable length, and divide it into three parts, which makes eight inches, or 96 lines [of course in English measure 80 lines] to each part. On the first division, reckoning from the pivot, I trace the little scale of one line to a fathom, which gives me 96. From the extremity of the small scale I begin the division Method of making the instrument,

* Journal des Mines, Vol. XXVI, p. 461. Extracted from a paper sent to the Council of Mines.

 of

of the larger, and the 192 lines remaining give me 64 fathoms each represented by 3 lines*.

I afterward subdivide each of these scales by three.

and using it. I fix together the plan and the paper on which it is to be reduced, the latter being under the small scale; and place the rule so that it can traverse circularly as much of the large plan as its extent will admit. The rule being fixed on the large plan wherever it touches a point to be transferred to the paper, I note the number of toises on the large scale, and opposite the same number of toises on the small scale I make a mark with the point of a needle set in a handle, or merely with a fine lead pencil. Thus I set down all the parts of the plan one after another, which are found just and in due proportion.

If the plan to be reduced exceed the length of the rule, the instrument may be removed to another place. I need not mention the necessary precautions in this case for placing the minudometer properly†.

A different construction. At first I placed my pivot between the two scales, counting the divisions in opposite directions; but as the plan was reversed in this case, I had not the advantage of comparing it readily with the original as I proceeded.

It is obvious, that, if we would have other divisions, we must have different rules, or trace these divisions on paper, and paste it on the same rule. The rule may be graduated also on both sides‡.

V.

* It might be supposed from the text, that Mr. de la Chabeaussière began to count the divisions of the large scale from this point; but this would be obviously wrong: both scales must begin their count from the pivot, consequently the first division in the larger scale must be reckoned, in the instance before us, as 33, so that both scales will end with 96. It should have been said too, that the pivot, or the hole for it, must be placed in a line with the edge of the scale carrying the divisions. C.

† Though the divisions of the scale amount to 96 toises, there are only 64 that can in reality be used. Consequently it must be necessary to shift the minudometer and the paper once at least. C.

‡ If the edges were bevilled in opposite directions, and the rule were in two parts, made to fit into each other either way where the smaller scale terminates; and the units were of different lengths, though similarly divided; this would give four proportions for diminishing or enlarging. If for instance the principal divisions of one of the large scales were an inch, and

V.

On Mortars and Cements; Experiments that show the Cohesion which Lime contracts with Mineral, Vegetable, or Animal Substances; extracted from a Paper read to the French Institute the 17th of October, 1808, by B. G. Sage*.

HAVING found, that an alkaline lixivial gas was evolved from a mixture of three parts of sand and two of lime slacked by immersion; and desirous of ascertaining, whether the products of the three kingdoms, mingled in the same proportions, would afford a similar gas; Mr. Sage made a number of experiments, which taught him, that the force of cohesion contracted by slacked lime was greater with metallic oxides in general, than with any other substance. These trials led him to new facts, which enabled him to discover mortars, or cements, at least as solid and impermeable as those made with the best puzzolana, which is of the greatest use, particularly in hydraulic structures.

Gas evolved from lime and sand.

Metallic oxides strengthen mortar.

The work we announce points out also a prompt and easy method of ascertaining the solidity and impermeability of mortars or cements, which cannot but be highly interesting to builders.

We must not always judge of the goodness of a cement from its having acquired a great deal of solidity in the open air, for it frequently loses this in water, in which it diffuses itself. Buildings made with such mortar soon tumble to pieces.

Mortar solid in the air may not stand water.

The necessity of a minute division of the substances, that enter into a cement, cannot be insisted on too strongly. They should first be mixed together uniformly while dry; and they must not be drowned in water, which must be added gradually, till the mixture is reduced to a soft paste.

Rules for making good mortar.

and of the other an inch and half; and those of the small scales, one half an inch, the other a quarter; we should get the proportions of a half, a third, a fourth, and a sixth. Two rules, with joints mutually fitting each other, would give 16 different proportions. If both edges be graduated, there must of course be a hole for a pivot at the extremity of each. C.

* Journal des Mines, vol. XXVI, p. 471. The above appears to be the title of a pamphlet, which Mr. Sage has published separately.

It

It is of the greatest importance to determine with precition the quantity of lime employed to obtain the most solid mortars or cements; and in general to use no lime but what has been made from pure limestone, and which has been kept well secured from the air after it is slacked.

Two parts of lime to three of other matter.

In the experiments of Mr. Sage he always employed two parts of lime to three of puzzolana, of sand, &c., which afforded him very hard and impermeable mortar: and he thinks this proportion of lime may even be lessened, when the architect is fully convinced of the impropriety of leaving the preparation of mortar to brickloyer's labourers, since the strength and solidity of hydraulic structures depends so much on it.

Mortars of lime and ashes.

The author has divided his experiments into five classes. 1. Mortars or cements made with substances, that have undergone the action of fire. The ashes of vegetables, whether lixiviated or not, being mixed with two thirds of lime slacked by immersion, forms one of the most solid and impermeable cements: a property which they appear to derive from the minutely divided quartz, which these ashes contain in the proportion of one fourth.

Lime and iron oxide.

Iron alone a cement.

2. Mortars or cements made with metallic substances. Iron adds to the hardness of all mortars; and of itself, in rusting, concurs in the agglutination of gravel and pebbles, as we see on the seashore. According to the state in which the iron is, that is combined with two parts of slacked lime, its force of cohesion is more or less considerable.

Lime and different stones.

3. Mortars or cements made with stones of different natures. Gestein, chalcedony, sandstone, and gravel, form very hard and impermeable mortar with lime. Feldspar, better known by the name of petuntze, being mixed with two thirds of slacked lime, produces an impermeable and solid mortar.

Lime and mould.

4. Mortars or cements that alter in water. Vegetable earth, or mould, is essentially composed of minutely divided quartz, clay, and iron. Mixed with two parts of slacked lime, and water enough to form a soft paste, the brick produced from it, when dried, has some solidity, which it loses under water, where it cracks.

Lime and

5. Mortars or cements made with combustible substances. Mortar,

Mortar, or cement, made with sulphur and two parts of slacked lime, forms a hard and very sonorous brick, which is not altered under water; while mortars made with pulverised vegetable charcoal, or pitcoal, though they produce hard and sonorous bricks, soon fall to pieces in water; as do bricks made with sawdust, or raspings of ivory.

combustible substances.

VI.

Observations on the Alkaline Metalloids: by Mr. BUCHOLZ *.

The quantity of metal obtained by means of iron varies.

THE quantity of metalloid substance obtained varies considerably. In an experiment made lately in my apparatus with three ounces of potash, six drachms of charcoal, and an ounce and half of iron, I obtained but one drachm of metalloid, divided into four or five pieces. In the tube were found thirty grains more of metalloid, clotty, and contaminated with charcoal; yet all the vessels had stood well, and remained impervious to air. The residuum, which furnished prussiate of potash, still contained however a large quantity of charcoal. It is clear therefore, from the small quantity of the product obtained, that it is not the whole of the charcoal, but perhaps only the hidrogen it contains, which concurs in the formation of the metalloid.

Its specific gravity ascertained by a mixture of lard and oil of petroleum.

Not being able to determine the specific gravity of the metalloid, as it alters so quickly in the air, I thought of composing an oil of the same density, in which it would neither sink to the bottom, nor float on the surface, and which consequently would be of the same specific gravity. This I did by mixing oil of petroleum and lard. The specific gravity of this mixture was 0·876.

Metalloid converted into potash increased in weight 0·2.

Twenty-five grains of the metalloid, converted into potash by water, and saturated with muriatic acid, produced 45 grains of fused muriate of potash, which, according to Rose's analysis, would contain 30 grains of potash and 15 of acid: but, as only 25 grains of the metalloid were employed,

* Ann. de Chimie, vol. LXXIII, p. 78. Translated from Gehlen's Journal for May, 1808, by Mr. Tassaert.

ployed, there was an increase of 0·2, which favours the opinion of the alkalis being metallic oxides; otherwise we must suppose, that this increase of weight arises solely from the water of crystallization.

Its combustion rendered limewater turbid, owing probably to carbon from the adhering oil.

Into a well closed bottle, containing four ounces of limewater, above half a grain of the metalloid in several globules was introduced. The combustion was effected very speedily, and the water was rendered very turbid every time the globules sunk down, as Curaudau had observed. It might be presumed therefore, that the metalloid contained carbon; but, as it is very difficult to separate all the adhering oil, it may still be supposed, that the carbonic acid came from this oil. I thought I should obtain a much more certain result, by converting the metalloid into an amalgam with mercury, and thus immersing it in limewater, which would prevent the combustion of the oil. In this process the evolution of gas was very brisk, without the water becoming turbid; but the gas gradually ceased to be evolved, and the surface of the amalgam became covered with a light gray pellicle, which rendered the fluid turbid and gray, but not milky. A few drops of nitric acid did not make this cloudiness disappear, and the mixture acquired a metallic taste. I poured distilled water on the remaining amalgam, and the evolution of gas commenced anew with a great deal of energy; but no pellicle was formed, and the liquid did not become turbid. This result may be explained on the supposition, that the contact of limewater favours in some degree the oxidation of the mercury; though it is not easy to say why this should take place, as it does not with distilled water, and accordingly no pellicle is formed. As no trace of carbonate of lime appears, it may be concluded, that the metalloid contains no carbon; but it would be well to confirm this by fresh experiments.

The combustion of its amalgam did not.

Amalgam differs according to the proportion of mercury.

On triturating one part of metalloid of potash with thirty of mercury in a porcelain mortar, a pretty ductile amalgam was formed, resembling amalgam of tin: but with ten or twenty parts of mercury a gray pulverulent substance only was obtained, which assumed a metallic brilliancy by pressure. On continuing to bray this substance, it became moist,

moist, formed at length an alkaline liquid, and the mercury became fluid. The tendency of this amalgam to combine with other metals is surprising, it combines even with iron at the instant of contact, and extends on its surface; but after some time the metalloid returns to the state of potash, and the mercury separates from the iron.

& has a strong affinity for other metals.

Twenty-five grains of the metalloid of potash being heated red hot in a narrow-mouthed vessel, the small globules united into larger, which had a bright metallic lustre, that was a mean between that of tin and that of silver, and were very fluid. On cooling they assumed the appearance of a hard amalgam of tin. In the open air they became covered at first with a gray coat, which became blue in a greater heat, and the blue colour of which grew much deeper, when the gray pellicle was removed from the melted matter. On heating it more strongly all the colour disappeared, and the whole assumed a silver whiteness, with a metallic lustre, which became gray on cooling. A little of the fused matter, being brought into contact with the air, took fire, and gave out a white vapour, not alkaline, which deserves examination. On heating it to a cherry red, a liquid matter was produced of a yellow brown colour, and destitute of metallic lustre, which gradually became of a blue green, and comported itself as a siliceous compound that attracted moisture from the air. Potash therefore was formed without previous inflammation, and the metalloid of potash had attacked the glass, agreeably to the experiments of Mr. Davy.

Potassium heated red hot in a narrow mouthed vessel,

and afterward exposed to the action of air,

and of heat.

Some time ago I treated alkaline matter, from which I had failed to obtain metalloid, with linseed oil, according to Curaudau's process*. Having subjected it to a very violent heat, I could obtain no fluid metalloid in the receiver; but in the neck of the retort I found a portion in clots, mixed with carbonaceous matter, weighing about two drachms. On heating it, and straining it through a rag under heated petroleum, I obtained half a drachm of liquid metalloid.

Potassium obtained by means of linseed oil.

The residuum still comported itself like the púre metal-

* See Journal, vol. XXIV, p. 38.

loid

loid with water, mercury, and other substances. The coally matter, from which the metalloid had been separated, appeared to me a detonating pyrophoric product of a peculiar nature. It had the following properties. Its colour varied from deep black to brownish black, and black blue. It had a greater or less degree of cohesion, a pulverulent consistency, but requiring the stroke of a pestle to reduce it to powder. The pulverulent part inflamed with noise on the contact of air; but the large pieces did not take fire, till they had remained exposed to the air some time. They inflame more quickly when moisture is near. On triturating, striking, or pounding this matter with a solid body, it detonates with more or less noise, with flame, and with dispersion of the matter when the pieces are large. The noise resembled loud cracks of a whip. I have even observed, that this decomposition of the metalloid with noise takes place sometimes under water, and occasions a violent commotion in it. This detonating product was near occasioning me as disastrous an accident, as the metalloid did Mr. Gay-Lussac; for, in attempting to get all the matter out of the neck of the retort with a sharp-pointed iron wire, a portion detonated with a great deal of noise, and almost all the burning matter flew by my face. It is obvious therefore, that we cannot be too cautious in operating on this substance *.

Dangerous accidents.

Action of potassium on oil of turpentine.

On another occasion I observed a very violent action from some coally matter filled with metalloid. I poured about a drachm into an ounce of rectified oil of turpentine, and immediately perceived a very strong ebullition of the oil, part of which was volatilized in smoke. What remained had almost entirely lost its smell, but had acquired a striking smell of solution of camphor in oil of turpentine, yet I could not by any means discover the presence of camphor.

Detonating mixture.

* A mixture of sulphate of potash and vegetable charcoal, in a large proportion, produces a similar effect. *Collet-Descotils.*

VII.

VII.

Farther Observations and Experiments on Oximuriatic Acid, by J. MURRAY, *Lecturer on Chemistry, Edinburgh.*

To Mr. NICHOLSON.

SIR,

IN a former communication I had given an account of some experiments, which I regarded as subversive of Mr. Davy's lately proposed hypothesis on the nature of muriatic and oximuriatic acids. Of these some of the results were called in question by that gentleman, particularly that in which carbonic oxide, hidrogen, and oximuriatic acid gasses were subjected to mutual action, either at a low temperature or by detonation. The production of carbonic acid in this experiment he appeared to have considered as arising from the operation of the water introduced with the view of examining the product; he employed therefore dry ammoniacal gas, and with this variation he stated, that there is no conversion of carbonic oxide into carbonic acid. Though satisfied, that there is little probability in the supposition of any fallacy from this source, I thought it right to repeat the experiment so as to exclude its operation, and having lately done so, I beg leave to communicate the result.

Result of the action of carbonic oxide, hidrogen, and oximuriatic acid, questioned.

I may previously remark, that I had objected to the imperfect manner in which Mr. Davy's experiment was executed; no attempt apparently having been made to discover if carbonic acid were formed, but its nonformation having been inferred merely from the residual gas burning with the same coloured flame as carbonic oxide. This has since been attended to, and the experiment performed with a more strict examination of the result. An account is given by Mr. J. Davy in his last communication of this repetition of the experiment. A mixture of 10 measures of carbonic oxide, 4 measures of hidrogen, and 14·6 measures of oximuriatic acid gas contaminated with 2 of common air, was inflamed by the electric spark; the residual air being detonated with oxigen was found to contain 8 measures only of carbonic oxide; 2 measures of this gas therefore had disappeared,

Mr Davy's experiments on the subject

appeared, and it appears to be admitted in the statement of the experiment had been converted into carbonic acid, as indeed no other conclusion could be drawn. But this is ascribed to the action of the common air, or of moisture in the gasses; and it is inferred, that, when the action of these is taken into account, "no result more satisfactorily conclusive that no carbonic acid was formed could be expected."

produced carbonic acid,

It is at least established, that in this experiment, when the results are submitted to accurate examination (even with the precaution, which Mr. Davy deems so essential, of substituting ammonia for water), there is a conversion of carbonic oxide into carbonic acid. The *fact* therefore is admitted, which I had asserted, and which had been before denied. The *suppositions* by which it is now attempted to be accounted for I regard as unsatisfactory, no proof being given, either that the causes assumed did operate, or were adequate to the production of the effect. With regard to the *supposed* operation of the atmospheric air mingled with the oximuriatic gas, it is not probable, that, diluted as it must be by the large intermixture of elastic fluid, its oxigen would combine with the carbonic oxide in the feeble inflammation, which from the small portion of hidrogen employed would take place in the experiment. And even if it had combined, the quantity of it was not sufficient to have converted into carbonic acid half the quantity of carbonic oxide which disappeared. With regard to the *supposed* effect from moisture, as the carbonic oxide and hidrogen gasses were previously dried, it can scarcely be assumed to have been present to the extent which it is necessary to suppose, allowing even that it could operate in the momentary action from the detonation. And if there were grounds for supposing, that these circumstances were of any importance in producing the result, why were they allowed to operate? It is easy to obtain oximuriatic gas without such an intermixture of common air as 2 measures in 14; it can also be dried by submitting it to the action of substances which abstract water. When they could thus have been excluded, the only reason that could justify this admission was the belief, that their influence was so unimportant

not satisfactorily accounted for.

portant that it might be disregarded. But to admit them, and at the same time to assume that their operation had given rise to the result, the possibility of obtaining which independent of such circumstances is the very question at issue, appears to be making by choice an ambiguous instead of a decisive experiment. I am satisfied however, that these circumstances had no important effect. And when we have the actual formation of carbonic acid, and only such modes of accounting for it to avoid the conclusion, that oxigen is communicated from oximuriatic acid, I cannot but regard the result as being in conformity with that which I have always stated to be obtained.

One other observation with regard to this experiment I find it necessary to make. In employing hidrogen gas to promote the action of oximuriatic acid on carbonic oxide, the proportion I used was equal volumes of the hidrogen and carbonic oxide, and in the repetition of the experiment with the view of ascertaining if the result I had stated were accurate it was to be expected, that the same proportion would have been observed. Mr. Davy in his former experiment used the proportion of 8 parts of hidrogen to 10 of carbonic oxide, a deviation of no great importance, and of which therefore I did not think it necessary to take notice. But he has now employed the proportion of only 4 measures of hidrogen to 10 of carbonic oxide. I know not what may have been the reason for this change of proportion, but it is obvious what effect is to be expected from it. I had found, that dry carbonic oxide gas, and oximuriatic acid gas, do not act on each other; and I had affirmed, that they do act, and that there is a production of carbonic acid, when a portion of hidrogen is added. According to the view with which that hidrogen was added, that of affording a certain portion of water necessary to the constitution of muriatic acid gas, the larger the quantity used, the conversion of carbonic oxide into carbonic acid by the oximuriatic acid might be expected to be more complete. Mr. Davy repeats the experiment with the view of disproving the result I had affirmed to be obtained; but he reduces the proportion of hidrogen more than one half; and from not attending to the effect of it, he withdraws as far as possible the

The proportion of hidrogen in them too small.

trogen arising from the action of the oximuriatic acid on the ammonia. When mixed with atmospheric air and kindled, it burned not with the blue lambent flame of carbonic oxide, but with the quick flame of hidrogen, and afforded by its combustion only a small quantity of carbonic acid. This residue of inflammable gas, while there also remained a small excess of oximuriatic acid, is probably to be ascribed to imperfect exposure to light.

The slow action preferable to detonation,

In performing these experiments I preferred the method of submitting the gasses to slow mutual action at natural temperatures to that of promoting it by detonation, both as capable of being conducted with more accuracy, and in itself more conclusive. In the mode by detonation it is necessary to operate over quicksilver, and from the action of the oximuriatic acid on the quicksilver it is more difficult to observe the phenomena of the experiment, and to estimate the results. In the slow action this may be avoided. We farther avoid any fallacy which may be supposed to arise from the high temperature in favouring the decomposition of any water that may be present. And the mutual action, from its continuance, appears to be more complete. I confirmed however the preceding results to a certain extent, by performing the experiment by detonation, the test of muriate of barytes indicating the presence of carbonic acid in the solution formed by the introduction of water after the ammonia.

but the latter gave similar results.

Experiments of Mr. Cruickshanks

Mr. J. Davy in his first reply to my observations on this subject stated, that he had repeated some of Cruickshank's experiments on the production of carbonic acid by the action of oximuriatic acid on the carburetted hidrogen gasses. When the experiment is made over water, some ambiguity may be supposed to arise from its influence. But even when it is excluded, a portion of carbonic acid ought to be formed from the agency of the hidrogen similar to that in the preceding experiments: and I did not make the experiment to ascertain this only from the uncertainty with regard to the existence of oxigen in the composition of these gasses, which, if carbonic acid were formed, it might be contended contributed to its formation. Mr. J. Davy however considering this source of fallacy as of little importance, performed

repeated by Mr. Davy.

formed the experiment, excluding water, and stated, that he "never obtained carbonic acid gas, though oximuriatic gas in great excess was employed." I alluded briefly in my reply to the source of errour whence this observation I conceived had arisen, and I now find my conjecture to have been just. I had found in the case of the production of carbonic acid from the mutual action of oximuriatic acid, hidrogen, and carbonic oxide, that no milkiness is apparent on the first or even the second transmission of the gas through lime water, the small portion of remaining muriatic or oximuriatic acid preventing the formation of carbonate of lime. I had no doubt that this had operated in Mr. Davy's experiment, especially as he laid stress on the very circumstance which would give rise to it, the great excess of oximuriatic acid employed; and I have found, that this is the case. One measure of carburetted hidrogen gas obtained by passing watery vapour over ignited charcoal, freed from any intermixture of carbonic acid by careful agitation with lime water, and afterward dried, was mixed with a measure and a half of oximuriatic acid gas passed over dry muriate of lime; the mixture was inflamed over dry quicksilver by the electric spark; the residual gas was transmitted first through water, and afterward through lime water; no milkiness was apparent in the latter on the first or second transmission, but on the third the surface became milky, the whole became turbid on agitation, and this was repeated on two or three subsequent transmissions. The production of carbonic acid was therefore not in the least doubtful*.

Mr. Davy.

Source of errour.

The experiment repeated,

and carbonic acid produced.

* The residual gas in this experiment burned with the blue lambent flame of carbonic oxide, and gave carbonic acid in its combustion. If it were found to be carbonic oxide, it would prove, on the supposition that the gas from humid charcoal after careful washing with water is a binary compound of carbon and hidrogen, that still more oxigen had been communicated from the oximuriatic acid, than had gone to the formation of the carbonic acid; or, if this were not admitted, the result would throw some light on the disputed question with regard to what are named the carburetted hidrogen gasses, whether oxigen exists in their composition; as it would render probable the opinion, that this gas at least is a ternary compound of carbon, hidrogen, and oxigen, in opposition to the opinion, that it is a binary compound of carbon and hidrogen.

Residual gas.

All the results confirm the former statement.

The results of all these experiments then, instead of invalidating, confirm what I have before stated. The sources of fallacy supposed to exist have been found to have no effect; and the more accurately the results have been examined, the more strict has been the coincidence with that statement. In all of them carbonic acid has been found to be formed, and Messrs. Davys appeared not to have obtained it in their experiments, because they did not look for it with sufficient care, or were not sufficiently aware of the fallacies, by which its production might be concealed.

On the other topics of this discussion I am pleased to find, that it is not necessary for me to enlarge; as, with regard to those of any importance, Mr. J. Davy has in his last communication either attempted no reply to my observations on his former statements, or the reply is in general such, that, with a few remarks, I willingly leave the decision to the judgment of those, who have given attention to the question.

Remark on the assertion, that Mr. Davy's statement is not hypothetical.

He still for example professes to maintain, that the proposition "muriatic acid gas is a compound of oximuriatic acid and hidrogen" is not an inference from the fact, that this gas is obtained from the mutual action of these two substances, but is the expression of the fact itself; that because they are the only substances concerned in the experiment, and it is equal in weight to the weight of them employed, "muriatic acid gas is not inferred, but immediately perceived to be a compound of oximuriatic gas and hidrogen, and that all the other cases are analogous." His brother's views therefore he contends are not hypothetical; and, if I fail in proving them such, I fail, he adds, altogether. I confess I have felt surprised, that this ground of defence ever has been assumed, and that Mr. H. Davy should have remarked, that I have mistaken his views in supposing them to be hypothetical, adding, that "he merely stated what he had seen, and what he had found." And although Mr. J. Davy might at first have adopted these sentiments, I had hoped, that the observations in my former paper would have convinced him, that this view was a hasty one, that these pretensions were too high, and that the subject might be presented under a very different aspect. If I have failed in

in this, I must despair of being more successful by any farther illustrations, and I feel indeed no desire to add illustrations on what appears to me too obvious to bear a moment's reasoning. I shall only present the subject under one other light; and beg to remind him, that the very possibility of the proposition being called in question without any doubt being expressed of the accuracy of the experiment on which it rests is a sufficient proof, that it is not a simple expression of the fact, as he and his brother suppose, but an inference from the fact. I should not involve myself in the absurdity, or rather in the palpable contradiction of denying, that muriatic acid gas is *obtained* from the mutual action of oximuriatic acid and hidrogen, and is the only sensible product of that action, while I did not call in question the accuracy of the experiment of which this is stated to be the result; though I feel no hesitation in denying (equally admitting the experiment) that muriatic acid is a compound of oximuriatic acid and hidrogen. I perceive an essential difference between these two propositions; the one (supposing the experiment accurate) is a simple expression of a fact; it will for ever remain true, be the progress of the science what it may, and no one who understands the terms in which it is expressed will call it in question; the other is an inference from the fact, which may be questioned, and may prove to be false. If Mr. Davy however can perceive no difference between them, he is right in maintaining, that his brother's opinion is a genuine theory. I trust I need not add, to avoid misconception, that I have admitted, that, were our induction to be restricted to this fact, the conclusion drawn by Mr. Davy, as it is the most direct, would be the most probable one; it is only when connected with the other phenomena to which it is related, that it becomes more doubtful; it then comes in contact with a different conclusion, which may be drawn, and which in relation to some of these phenomena has in its turn the advantage of being more directly inferred; the two are to be compared in their whole extent, and the one which in its application to all the phenomena shall appear most probable is to be preferred. It is altogether a limited view, to look only to the experiment of the production of muriatic acid gas from the mutual

Remark on the assertion, that Mr. Davy's statement is not hypothetical.

mutual action of oximuriatic acid and hidrogen; for, to draw the conclusion from that experiment, we must previously know what is the constitution of muriatic acid gas, and what the constitution of oximuriatic acid; and the most probable inferences with regard to these must regulate the conclusion that ought to be drawn. If there is reason to believe, that the former is the real acid, and that the latter is a simple substance, it may be inferred, that muriatic acid gas is a compound of oximuriatic acid and hidrogen. But if there are facts whence it can be inferred, that muriatic acid gas contains water, or that oximuriatic acid contains oxigen, the theory of the experiment must be given in conformity to these—the oxigen of the oximuriatic acid combining with the hydrogen, and forming water, which the muriatic acid holds combined with it in the elastic form. There are facts from which these are the most *direct* and *probable* conclusions; and these conclusions are avoided by less direct, and more complicated and hypothetical assumptions. And it is merely an errour in logical deduction to suppose, that such assumptions require no independent proof, but are established because they would follow if the inference were admitted, that muriatic acid is a compound of oximuriatic acid and hidrogen.

Most obvious conclusion not always just.

To some of the examples which I had given, illustrating the general proposition, that the most obvious conclusion from an experiment is not always the just one, Mr. J. Davy has stated some objections, which perhaps it is superfluous to notice; for, were even the illustrations incorrect, the proposition itself cannot be denied, and it might be easily illustrated by other examples. The truth however is, that the examples I have given remain in full force. To one of them indeed, that from the production of dry muriate of potash, no objection has been offered. With regard to the other, that of the production of calomel by combining muriatic acid and oxide of mercury, there may be, as he supposes, a production also of water, (though this remains to be proved) yet still the most direct inference from the experiment is, that calomel is a compound of the oxide and acid; for it is a more simple conclusion, that this water had been deposited from the acid, than that it had been formed by

Production of calomel.

the

the oxigen of the oxide combining with hidrogen from the acid, while the oxmuriatic acid combines with the metallic mercury to form the calomel. Mr. Davy seems to doubt indeed if calomel can be formed by presenting muriatic acid to oxide of mercury. If the metal is highly oxidated, corrosive sublimate, it has long been known, is formed; but it is equally true, that calomel is the product of the mutual action of muriatic acid and mercury in a low state of oxidation.

I had given as an example of hypothetical assumption in Mr. Davy's system, the explanation of the production of oximuriatic acid by distilling muriatic acid from oxide of manganese; the explanation *supposing*, that the oxigen of the oxide combines with the hydrogen of the acid and *forms* water, while the oximuriatic acid is set free. To this Mr. J. Davy replies: "Mr. M. asserts, that Mr. Davy is obliged to *suppose*, that water is produced in the common mode of making oximuriatic gas from muriatic acid by means of the black oxide of manganese. Mr. Davy has ascertained the *fact*, that oximuriatic gas and water are produced when black oxide of manganese is heated in muriatic acid gas." It is almost superfluous to remark, that here the leading term in the proposition, and on which the whole discussion rests, is changed. I had asserted, that Mr. Davy is obliged to suppose, that in this experiment water is *formed*: and the assertion is strictly correct. To say, that Mr. Davy has ascertained the fact, that water is *produced*, is saying nothing to the point. The *production* of water in an experiment is not its *formation*, nor is it a proof of it; it is as probable *a priori*, that it is deposited, as that it is formed: unless there be particular evidence indeed for the latter conclusion, the former is to be preferred as more simple and direct; and though water is produced, in other words becomes sensible, when muriatic acid gas acts on black oxide of manganese, I repeat, that Mr. Davy is obliged to suppose it is formed; and that he has no other proof of its formation than the supposed truth of his hypothesis, which is of course assuming the point in dispute.

Production of oximuriatic acid from muriatic acid and oxide of manganese.

In a different part of his reply Mr. J. Davy, from not attending to this distinction between the production of water and

Distinction between pro-

duction and formation.

and the formation of water, has supposed, that I have neglected its agency in an experiment, when I only suppose the most direct conclusion, and the one most strictly analogous to that which would be formed in similar cases, to be drawn. If muriatic acid gas in acting on a metallic oxide disappears, forming a solid product, while water is also produced, the most obvious and direct conclusion, and the one most conformable to a very extensive analogy is, that the acid has combined with the oxide, and that the water has been previously combined with the acid, but does not enter into the new combination. If nitric acid vapour, or sulphuric acid vapour, were transmitted over a metallic oxide, with similar results; the disappearance of the acid, the formation of a solid product, and the production of water; this is the very conclusion which I suppose Mr. J. Davy would consider as the legitimate one; and it may be well for him to consider a little farther the grounds on which he violates this mode of induction, in refusing to draw a similar conclusion with regard to muriatic acid.

Hypothetical explanations in Mr Davy's system.

I had given some examples of hypothetical explanations in Mr. Davy's system, which I regarded as much more complicated than any of those which are given in the opposite opinion. Mr. J. Davy has endeavoured to render them more simple, but I fear with little success. Dry muriate of potash is regarded as a compound of oximuriatic acid and potassium. On dissolving it in water, I conceived, that, in conformity to the system, it was supposed to be converted into a compound of muriatic acid and potash, a portion of the water being decomposed, its oxigen communicated to the potassium, and its hidrogen to the oximuriatic acid: and that again in expelling the water from this solution, and obtaining the dry salt, the hidrogen of the acid and the oxigen of the potash combine, forming water, while the oximuriatic acid and the potassium enter into union. In giving these as the explanations which are conformable to Mr. Davy's system, I believe I have done it justice; and that, though sufficiently hypothetical and complicated, they are the most probable of which it admits, and are in conformity to his own statements: "the action of water on these compounds which have been usually considered as muriates,

muriates, or as dry muriates, but which are properly combinations of oximuriatic acid with inflammable bases," being stated to be exactly that which I have described; oxigen being supposed to be communicated to the base, and hidrogen to the oximuriatic acid. Mr. J. Davy rightly remarks, that they are *conjectures*: and to avoid them, he satisfies himself with remarking, that "fused muriate of potash is a compound of oximuriatic acid and potassium; and the solution of muriate of potash is a compound of oximuriatic acid, potassium, oxigen, and hidrogen." If fused muriate of potash is a compound of oximuriatic acid and potassium, how can it be obtained from the watery solution formed by uniting potash and muriatic acid, by the evaporation of the water, without the very changes taking place, which I have stated? With regard to the changes that occur on dissolving it in water, Mr. Davy gives a view indeed somewhat different from that which I had stated, but not much to the advantage of his hypothesis. It is not merely it seems a portion of water that is decomposed so as to form muriatic acid and potash; but the whole water, if the above statement of Mr. Davy with regard to the nature of this solution have any distinct meaning, is decomposed; and the solution is a quaternary compound of potassium, oximuriatic acid, oxigen, and hidrogen, in which neither potash, muriatic acid, nor water exists. If this is simplifying chemical theory, and rendering it more probable, we have hitherto been much mistaken in our notions of simplicity and probability. I know not if Mr. J. Davy is prepared to extend the same view to other analogous cases, and to say for example, that, when we dissolve sulphate of soda in water, we form a compound of sodium, sulphur, oxigen, and hidrogen. It is needless to analyse the other example of the mutual action of nitrate of mercury and muriate of soda, as the same remarks nearly apply to it.

Nonaction of oximuriatic acid on charcoal.

I had stated the fact of charcoal not being acted on by oximuriatic acid as presenting an anomaly in Mr. Davy's hypothesis; for, since oximuriatic gas is held to be a principle similar to oxigen in its general chemical agencies, it ought like oxigen to combine with charcoal, and still more so, since it combines with all other inflammable and metallic

tallic substances. Of this a satisfactory explanation may be given in conformity to the opinion, that oximuriatic acid acts on inflammables by imparting oxigen; while it remains an anomaly in the opposite opinion. To this the reply is made by Mr. J. Davy, that I seem to consider every thing anomalous, that is not accounted for; and the query is added, "Can Mr. M. account for the want of action between charcoal and nitrogen, and between the metals and nitrogen? and, if he cannot, does he consequently consider these facts anomalous?" The fallacy of this reasoning I should scarcely have supposed could have escaped observation. The anomaly with regard to charcoal is not simply, that it is not acted on by oximuriatic acid, as it is not acted on by nitrogen; but that, being an inflammable substance, and every other inflammable being acted on by oximuriatic acid, it is not. Inflammable substances are not acted on by nitrogen, we have therefore no reason to expect any action to be exerted by it on charcoal; while there is reason to expect, that charcoal, in common with other inflammable substances, should be acted on by oximuriatic acid; in the one case there is no general result, to which an exception occurs; in the other there is, and there is therefore an anomaly. Of this singularity with regard to charcoal, the explanation which may be given in conformity to the common opinion is so satisfactory, as to afford even a presumptive proof of the truth of that opinion, the fact being precisely what might be expected to occur. On Mr. Davy's hypothesis it is confessedly incapable of being accounted for.

New gas observed by Mr. Davy.

With regard to the new gas, which Mr. Davy has observed, a compound, as he regards it, of oximuriatic acid and oxigen, I have little to say. Without speaking lightly of it, as Mr. J. Davy imagines; or without doubting, that it may be able to convert carbonic oxide into carbonic acid; I may simply remark, that I have no reason to believe, that it operated in my first experiments; it no doubt was excluded in the repetition of the experiment by Mr. H. Davy, in which, as has already been remarked, carbonic acid is formed; and I have farther avoided it in the experiments stated in this communication, without finding any difference

tuces the results. The difficulties which Mr. Davy has supposed attend the common opinion from the comparative inactivity of this gas, though it contains more oxigen than oximuriatic acid, and which he imagines will probably lead me to "adopt the new idea, that oximuriatic gas is a simple body," appear to me of no weight. The powerful action of oximuriatic acid does not depend merely on the quantity of oxigen it contains, but on the state of combination of that element, and the disposing affinity exerted by the muriatic acid; and I can easily suppose the quantity of oxigen to be increased without any augmentation, or even with a diminution of power. It will be time enough however to explain this to Mr. J. Davy, when the properties and composition of this new compound are more fully detailed.

I have now given all the attention to this controversy, which it appears to me to claim; and the progress of it has, I trust, shown more clearly, that the common opinion of the relation between muriatic and oximuriatic acids is still the most probable one, inferred by the most simple and direct induction, and in strict conformity with the established theory of acidity, and the chemical agencies and combinations of acids; while the opinion of Mr. Davy, instead of being, as it has been contended, a simple expression of facts, is an hypothesis, involving assumptions gratuitous and complicated, and at variance with extensive and well established analogies. The experimental proof I have brought forward, and which I consider as sufficiently confirmed, is still farther, it appears to me, conclusive in support of the opinion I have maintained. I regard the discussion on my part as closed, and I shall not be disposed to resume it, unless some new facts or arguments are adduced sufficiently important to demand consideration.

I am, with much respect,

Your most obedient servant,

Edinburgh, June the 7th, 1811. J. MURRAY.

P. S. Mr. J. Davy, in a communication in the number of your Journal for May, has stated a series of facts, from which

On the nature of the metalloids.

which he has inferred, that the opinion I had advanced with regard to the nature of potassium is unfounded. He will have observed, that, in my second paper, published in the supplement to your last volume, which accompanied that number, I have taken notice of the greater number of these facts; and, that I had given them due consideration both in conducting the additional experiments of which I have given an account, and in forming the conclusions I had drawn. It is therefore unnecessary for me to make any observations on his statements in their present form. The whole subject, from the difficulties which attend it, I consider as open to further investigation, though I may add, that, without placing any undue confidence in my own experiments, I do not consider their results as invalidated; and, that I still regard the view I have given of the nature of the metalloids as the one which is most probable; nor shall I have any hesitation in engaging in the most minute discussion of the grounds on which it rests.

VII.

Description of Firs, illustrated by Dissections. By Mrs. AGNES IBBETSON.

To Mr. NICHOLSON.

SIR,

Arrangement of the fir tribe.

I AM now to give a description of the fir tribe of plants, seldom, I believe, studied, though well worthy of attention, as differing more in many important particulars than any natural order of plants I am acquainted with. Though seldom interfering in the arrangements of botany, I have ventured to place the thujas with the cypresses, allotting the cedars to the genera they appear to belong to. For they have been hitherto placed without the least regard to their flower or fruit; else could the white cedar be called a cedar, or the balm of gilead fir a pine? I shall divide them into three sorts, the pine, the cypress, and the cedar, placing the various species according to their fructification.

Firs

Firs differ from plants in general in having no spiral wire; for these vessels are absolutely only to be found where the leaves require turning, and not when so fastened on the main stem, as to be incapable of changing their position: an arrangement that might have been expected, since to turn the leaves as habit requires, to open and shut the flower, are the real offices of the spiral wire. They have no spirals.

The fir tribe differs also in forming their bark and rind by leaves; for, while in common plants the juices with the threads or vessels of the bark form together the upper covering of the stem, in the firs they form leaves alone; and with these the tree is covered. The leaves of the pines are more simple in their formation than leaves having the spiral wire, all the rolling and pressing is not used in any of the fir tribe, though the buds are more difficult to be understood in their general arrangement. To comprehend a leaf bud when forming, you must take it out of the interior of the "leaf calyxes," within which, and next the stem, it will be found. They also form their bark and rind from leaves.

The leaf bud consists of several pairs of calyxes, having a bundle of leaves weaving: as at fig. 1, Pl. V. Take one of these, and in the solar microscope it will show a very curiously worked wood, vessels ready formed; as a middle to the leaf, and a parcel of threads weaving the sides of the leaf by passing backward and forward: see fig. 2, where *a a* are the sides, and *b b* the middle, through which the threads pass. When this is done, the pabulum or blood of the plant coagulates, and settles on the threads, forming a mass both thick and durable; while the cobweb skin, which is woven with the calyxes, fastens on it, and covers the whole. Now the edges of the leaf begin to shoot, while threads of singular fineness and beauty appear; but scarcely have you time to admire the various prismatic colours they reflect in the sun, ere they are covered by the same cobweb skin, which makes of these apparent glass rings (for such they seem) one regular circular vessel; bordering the leaf, and fastening down the upper surface: the next appearance of the leaves is at the top of a bud, their form is then complete, though extremely small. In this bud you see the first starting of the flower bud from the line of life as at fig. 3; where *c c* are the female buds, *d d* the leaves. No sooner has the The leaf bud.

flower

flower bud got its scales and clothing to fit it for the cold it may encounter, than the stem will lengthen, and leave the female bud on each side of the stalk; carrying its leaves (still covered by a scale) on the top: when growing in length, and having now acquired a proper height, for the last time the stem begins to shoot, and the leaves push off the scale as they increase, depositing their proper number at each point of the stalk, according to the species. But the calyx still remains stationary, so that the length of the stalk, with the number of leaves contained in each bud, is easily known.

Peculiar bud in the pines.

There is in the pines a peculiar sort of bud, that must catch the attention of the most careless. In the shape, and with the appearance of a bud, it is in reality the spring shoot, showing itself in May or June, just after the leaf buds have made their spring increase, and when their feathery tops display such beatiful green plumes. It is also that peculiar thing, which serves to show the height the tree gains each year, and proves, that the leaves alone form its covering. It is the increase of the stem without the wood; that is, the bark and inner bark forming their shoots, while all around the sides, closely imbedded, are found buds of leaves, serving, as the stem increases, for the future covering of the tree. As soon as this is finished, the wood, line of life, and pith shoot up in the middle, and then the stem is completed.

Female bud.

But this does not happen till the female bud is formed at the top of this new shoot. At first the line of life runs up through it, and may be seen as a few green threads, followed by some wood vessels. The female flower is then protruded; and the rest of the wood begins to grow. This is an uncommonly curious process, as plainly proving two things: 1st, That the bark, inner bark, and leaves, want little assistance from the wood: 2d, That as soon as the pistil and stamens begin to grow, the line of life is their first accompaniment, and then the wood. The bud, when the female cone appears at top, is near a foot long, and often more in the Scotch fir, in the spruce still more, and in the silver fir less. Still it is the same thing, though rather different in appearance.

There

[redacted]

Peculiar bloom in the Scotch fir and Weymouth pine, caused by a cryptogamous plant.

There is a peculiarity in the Scotch fir, and Weymouth pine, not to be found in any of the firs, I mean the beautiful matter, which resembles the bloom of a plum, and which, like that, is a cryptogamian plant of an elegant kind; and though its extreme thickness grows only in spots, yet it is spread in a less degree over all the back of the leaf. It comes not till the leaf is fully formed; and disappears with age and sickness.

Scotch fir.

The Scotch fir is very different from the other pines in growth. If not in perfect health, and in a soil exactly suited to it, it is but too apt to grow squalid and ugly. Indeed no trees so directly show sickness as the firs. As soon as the stem of the side bough ceases to be on an even line with the branches that proceed from it, especially at its termination, and as soon as it stands much above them, it begins to mark a disordered frame, and its future symptoms of decay are as regular as the seasons. For years, the tree will continue growing more unsightly, though it may require a century to kill it. But when in perfection it is a beautiful tree, and less formal than other firs. The pinus latifolia is a variety of this species.

Marks of disease in it.

Peculiar matter in all firs, and in all trees that yield tannin.

The stem of the pine I have in part described, the leaves standing instead of bark and inner bark; the scales instead of rind. But next to the bark is a matter in all firs, which has hitherto been called by that appellation, though differing entirely from it. To inquire into the nature of this substance, its use, and why placed there, may be worth the trouble. On examining all those trees which have hitherto yielded the tanning principle, I find they have invariably this substance placed next the bark, and joining the albumen; although it is fonnd in no other trees. On farther examination it appears to be allotted to them in a degree of thickness very nearly proportioned to the strength we have found in this same tanning principle, in each tree. Thus in the sumach it is composed of about 8 or 10 rows in thickness, in the oak of 6 or 7; in the willow of 5 or 6: and so on. Now on placing a piece of this matter in the solar microscope; I find, instead of being bark, it is wood formed exactly the same as the wood on the other side of the albumen. But so altered, so changed in its appearance and

feel

feel, that a large magnifier alone could prove it the same: for instead of that hard and harsh substance, it is soft, smooth, and pleasant to a great degree. But when I came to dissect the firs, instead of finding a few rows of this matter, there were 40 or 50, making two or three tenths of an inch in thickness; it was become so soft in every respect, that it serves for bread in some countries. Though so thick it will turn round the finger with the utmost ease, and is far more succulent, more oily, and of a more beautiful white colour, than this matter in any other trees I have mentioned.

Use of this matter.

From all those observations, I think I may notice the conclusions I have drawn from these data, without being accused of giving way to imagination. I am persuaded, that this matter, placed in this situation in the tree, is intended to guard the albumen from being steeped in this softening liquid, and therefore never gaining the strength requisite to it: that the matter thus placed shows the effect of this tanning principle by the extraordinary changes of its appearance: and that the conclusion naturally to be drawn from the whole is, how much stronger must the tanning principle be in the firs, when nature is forced to have recourse to such an expedient, to such a treble guard: and how strong must the juices be, which have produced so astonishing an alteration, for the wood can only be compared to beautiful white leather. Why this matter should tear off with the bark, and leave the wood, is easily explained, as is also the reason why at this time the bark comes off at all. It is in the spring and fall, that the new albumen shoots; and it is then so soft and watery, and its vessels, if formed at all, so weak, that the smallest effort separates them. Indeed, at first it is only a collection of the sap to form the albumen; and they of course then fall apart.

Wood in other trees.

As to the wood of the pines, it is nearly the same as in any other trees; composed from the depositions of a new row each year.

Fructification of the pines.

I shall now show the fructification of the pines. There is perhaps no seed, where nature so plainly and openly exposes her whole process, as in this tribe of plants. So evidently indeed does she develope them to the view of the at-

tentive

tentive physiologist, that even dissection is unnecessary. I shall also, in describing the seed, prove the truth of all I have hitherto advanced on this subject, and shall continue to take my specimens of the pines from the Scotch fir. There is a curious particular concerning them, yet unknown I believe. The cones of the present year are not impregnated till the following; nor are they fit for planting, or will they come off the tree, till the succeeding season. When they are first seen on the new shoot, the stamens have already exhausted all their powder: besides, the cones have at that time no seed within them. But the following May, as soon as the stamens make their appearance, the cones, if watched, will exhibit a beautiful sight. On each squama will be seen two brilliant drops of liquid, the juice of the pistil, appearing toward noon, and subsiding in the evening. For a little time it will continue thus, till the stamen has risen out of its calyx, and each anther hangs like a basket of gold dust, ready to disperse in air. In a short time the drops on each pistil get saturated, and pass down to the seed, which they impregnate; running the line of life, filled with the mixed liquor, into each seed, and forming the corculum. As soon as the heart is perfected, the same line shoots lower, and produces the pocket, which is the outward cuticle of the embryo, and the cotyledons. When the pocket is large enough it joins to the heart, and the cotyledons begin to grow; and this is a long process in the fir tribe of plants, where there are from 5 to 10 in each seed. I know no plants so capable of proving the mistake into which most botanists have fallen, "in supposing the cotyledons nourish the embryo"; for though these seeds, like all others, have the 8 parts perfect; yet, being of the foliferous kind, they are so very diminutive, a large magnifier is required to see them. Would then most nourishment be formed, where there was hardly any embryo to feed? Besides, as I have before observed, the cotyledons are a part of the embryo; it would therefore be nourishing one part with the other; an idea not to be supported. In the firs also the nourishing vessels are so very plain, that all must see them. See fig. 4 and 5.

The cotyledons do not nourish the embryo.

Having now explained the Scotch firs, as an example of all

The cypress kind,

all the pines, which they in fructification and habit closely resemble, I shall turn to the cypress kind; including only those, the fruit of which bear a strict analogy to the cypress; as the white cedar, the balsamea, the arbor vitæ, and others, too many to name: taking the white cedar as an example of all the rest.

instanced in the white cedar.

The young shoot.

The young shoot of the cypress kind is curious. It so much resembles the juniper, that the most knowing gardener would be deceived. This is caused by the first shooting of the leaf bud in the axil of the leaf: which necessarily throws it from the stalk, to which at every other time it cleaves most closely: for they have imbricate leaves, with the leafing branches quadrangular, which makes them take a pyramidical form. The Bermudas cedar is only a variety of the cupressus sempervirens, expands more in its branches, grows larger in size, and is that species from which the wood is taken, so remarkable for its resistance to the insect tribe. The pinus balsamea, with its brown and woody corollas, has the same fructification, though the cone is in the former more expanded. In the arbor vitæ it differs little; though this has generally been supposed to carry its male and female flowers on different trees. But this I conceive a great mistake; I have repeatedly drawn them from the same plant, as well as in the balsamea.

Bermudas cedar.

Balm of Gilead fir.

Thuja.

Stems of the young branches.

The stems of the young branches of all these of the cypress kind are more formed like leaves than stems, only that they are so thick as to have no edges. They are almost wholly composed of pabulum, having very few regular vessels. A quantity of smaller bubbles of resinous matter, surrounded by a net work enclosing now and then a larger circular bleb. Thus net on net appears to form both the minor branches and leaves: but the principal stems are composed as those of firs in general; except, that in the larger stem of all firs there is a peculiarity not yet noticed. In showing the interior formation of trees, I mentioned the grand obstruction, and the middle*. This last was the stoppage of the pith at the commencement of each branch. Now when

Principal leaves.

Peculiarity of the firs.

* See Journal, vol. XXVIII, p. 259, 260.

when a branch is divided in the firs, the wood as usual is perceived to supply the place of the pith; but in the middle of the wood is a square of pith proportioned to the size of the branches, which is seen in the firs only. In all firs there is very little pith: possibly therefore it may be intended to supply the moisture necessary to raise the wood for the passing of the buds: for in the firs almost all the buds may be seen passing from the line of life to the exterior in this very place; and perhaps no plants give more complete conviction to the mind respecting that important point, namely, whence the flower buds proceed, than the firs; for they are seen proceeding in every direction from the interior, and throwing off their female cones as the stem increases.

The leaves are formed with a *large bladder* in the middle, and a thorn at top. The leaves.

The fructification is very different from that of the pines. Fig. 7 is a single squama of the cone of the cedar thyoides; fig. 8 is a squama dissected; and fig. 1, Pl. VI, is the male ament. The fructification.

I now turn to the real cedars, at the head of which may be placed the cedar of Lebanon. With these I have joined the larch, and all those the leaves of which grow in bundles. In fructification they much resemble the pines; but their nature agrees not together; and if any should be separated beside the cypress, it should certainly be these. They are hardy, and brave every climate, from the hot Bermudas to the moist Barbadoes, and the cold New England, and grow in perfection in all. They grow also in the bogs of America, and on the mountains of Asia. The cedar we have from Jamaica is a spurious sort; and the wood so porous, that wine soaks through it; while that of Carolina (probably a true cedar) is so firm and close, that it often preserves the strongest spirits in vigour. In this country none of these firs have any scale, or covering to their leaf buds; and they are also perfectly alike in their manner of forming their leaves. It is curious, that in the pines, where the leaves are few, or in pairs, they weave in bundles; and in the cedar, larch, &c., where they come out in bundles, they weave singly. There is no apparent leaf bud; the whole work is formed within. Each separate little calyx has a bundle of threads, which it winds round the long vessels, Cedars. Leaves.

vessels, working them in and out like basket work, thus binding them to the middle wood vessel. But no sooner are the leaves formed, though ever so diminutive, than the stalk shoots, carrying up the leaves with it, and another general calyx forms round the parcel of leaves; the single calyxes remaining to bring out fresh ones, and to serve to cover the new stalk. The edges of the leaves are formed very differently from those of the pines. A parcel of threads, very clear, and apparently full of water, are found shooting by the side, and binding themselves to the leaf by a single thread. In some of the real cedars there are two, in some three, and in the larch four of these vessels.

Peculiarity in the cedar of Lebanon.

There is a peculiarity in the cedar of Lebanon so very extraordinary it must not be passed over. The upper covering of the trunk of the tree seems as if too long for it, and sits in high ridges all the way, appearing as though, if stretched out in length, it would be as long again. It would be very instructive to know whether this is the case in its native land. I have long been seeking for the ball and socket found in some plants, and peculiarly marked in some firs, where the branches have missed. In the cedar I was much struck with this appearance, and resolved to try whether I could find the ball. On cutting round it, it moved under my hand, and I found it was easily taken out. I have now procured ten of them, some formed like a pointed top, some merely circular, but the bark and rind, instead of being, like that of the rest of the tree, formed of thickened leaves, are divided into narrow slips of bark and rind, rolled, and covering it like basket work.

Another peculiarity.

There is also another peculiarity never seen in our forest trees, and which appears to belong only to the exotic trees: a projection round the part where the branches first shoot. If they have it not in their own climate, it may be an increase to strengthen them, weakened by growing in a foreign country.

Fructification.

I shall not occupy your pages with describing at length the fructification of the cedar, as its process very nearly resembles the account already given; but mention only, that its cone is extremely large and solid, and appears to contain a greater quantity of the tanning principle than any other

Abundance of tannin.

other part. The seeds are not only full of it, but are covered without by bladders filled with the same juices.

As I have now concluded my account of the firs, I shall finish with a few words respecting wood in general, as one of the most important subjects in the botanical world. Some of our best physiologists have made a strange mistake, if I may venture to say so, in supposing it impossible that the wood can convey sap, because the wood can be torn to atoms. Look in the microscope at one of these shreds, and it will be found pointed, not a sap vessel, but a fragment. The sap vessels are round, but the wood has besides the bastard pipes, pieces of thin flimsy texture, which fill up all the places between the sap vessels, and are very large in young wood, and will divide into hairs; which are often taken for important vessels. The sap vessels also will separate, but I cannot conceive their being thus flexible, and easily torn, lessens their power of conveying sap when perfect and whole. But is it not an easy thing to prove, that they are the real sap vessels? since they are the only pipes yet found in plants, that will convey coloured infusions, as all acknowledge. I have repeatedly taken a branch three or four feet long, and though I could not make the coloured mixture rise the whole space at once; yet by cutting a little below where it stopped, I have made it by degrees rise the whole length, and thus proved, that there is no real stoppage in the vessels; but that the sap is capable of flowing in one even current from the bottom to the top of the tree; and the only reason we cannot make coloured liquids rise with the same ease and quickness as the sap is, that our mixtures are not so well tempered as Nature's: there is always some dust, some matter to choak these little pipes. I once made some very curious experiments on capillary attraction, in very diminutive glass pipes, which rendered this most evident, not only between the liquids, but between the pipes which we make when compared to the perfect works of Nature.

Wood capable of conveying sap.

As I cannot believe, that any one can strip off the bark of a tree, and yet be doubtful whether the flower buds come from the interior of the wood, I am very anxious to persuade the physiologist to study at this season the tree

Flower buds from the interior of the tree.

newly barked. There he will not only see the buds just breaking through, but the variation in each different tree in this respect—the manner in which each point of the compass is marked by its growth, by the scarcely undulating line of the sap vessels in the north, and by their never ending half circles in the south.

I am, Sir,

Your obliged servant,

AGNES IBBETSON.

Explanation of the Plate.

Pl. V, fig. 1. A bundle of leaves taken out of the inner leaf bud of the Scotch fir, while weaving; with their calyxes.

Fig. 2. A single leaf much magnified, and showing the manner of forming all the leaves of the pines.

Fig. 3. A sort of general or mixed bud in the Scotch fir, when the leaves, *d d*, are completely formed, and they are discovered at the top of a bud; while the female cones, *c c*, are shooting from the line of life, though not one in ten lives to come out of the cradle in the bark, *p p*.

Fig. 4. The squama taken out of the Scotch fir. *a a* the pistil: *o o* the two drops: *b b* the line of life running to the seed, and entering it, to form the heart at *f*: *c* the nourishing vessels entering the seed at *d d*: and fastened to the cone at *e e*.

Fig. 5. The male collection of stamens or catkin.

Fig. 6. A single stamen with its scale.

Fig. 7. A squama of the cypress kind, taken from the white cedar.

Fig. 8. The same dissected: *h* the pistil: *i i* the drops appearing to catch the powder: *k k*, the line of life passing into the seed at *r r*. *l* the nourishing vessels passing into the seeds at *n n*, and then joining the cone at *m m*.

Pl. VI, fig. 1. The male catkin of the cypress kind.

IX.

On the Motion of the Flower of the Barberry. In a Letter from Mrs. AGNES IBBETSON.

To Mr. NICHOLSON.

SIR,

AS the berberis has been the subject of a letter from one of your correspondents, I have waited till the flower was in full beauty, to send you a sketch of the manner in which the whole motion is managed by the spiral wire. Dr. Smith has most properly observed, that it is a contraction of the stem (for it can hardly be called a filament) of the stamen: it is so; for the contraction is in the spiral wire within this stamen stalk, which is gathered up, as may be plainly seen, when put into the solar microscope. Pl. VI, fig. 3, *b b* is the corolla, *a a* the stamen fastened to it—it has also a fastening to the pistil, which, crossing from each side to the pistil and round it to the other stamen, makes a general communication, but not a very sensible one. The strong spiral wire, *o o*, that manages the flower, runs through the middle of the stamen *c c*, with two joining from the sides *d d*, and running into the nectaries, *f f*, in which they are fastened at *g g**. So uncommonly strong is this spiral wire, that it is larger than that which manages many flowers of three times the size. There are many cross spirals, for it is rather a complicated management; but this will at least account for the contraction that takes place from *a* to *a*, and is plainly to be seen. The same thing happens in the stalk of many plants when in bud, which alter their position when full blown. On placing a bunch of these flowers under water, it is very difficult to make the water get to them; but if they are once thoroughly wet, they move no more. However I again repeat what I have said in my last letter, that I cannot say I am myself convinced what is the

Motion of the flower of the barberry.

The flowers will not move under water when thoroughly wet.

* Pl. VI, fig. 2, *a a*, shows the stem of the stamen of the barberry in its perfect state; and fig. 3, *a a*, the stem cut open to expose the cases of the spiral wire.

power,

wer, which rules the spiral wire: but that this wire is the use of the motion, whatever may be the superior cause that regulates it, I am hourly more and more convinced. The berberis is curious on another account; its corolla is very peculiarly made, something like the watery corolla, but not quite; no one can look at it, and not see that it is water, which causes all the beauty of its light and sparkling appearance.

Structure of its corolla.

I am, Sir,

Your obliged servant,

AGNES IBBETSON.

X.

An improved Method of cultivating the Alpine Strawberry. By THOMAS ANDREW KNIGHT, *Esq. F. R. S., &c.**

Culture of the Alpine strawberry.

THE Strawberry is a fruit, which is agreeable to the palates of so many persons, and which disagrees with the constitutions of so few, that any means of improving the culture of it, and of prolonging the season of its maturity and perfection, will probaby be acceptable to the Horticultural Society: I am therefore induced to send an account of an improved method of cultivating the Alpine strawberry, that is, I believe, little, if at all, known, and that I have practised with the best possible success.

Valued as an autumnal crop.

Experiments.

Though the flavour of the Alpine varieties is generally approved, they are not much thought of, while the larger varieties continue in perfection, and are valued only as an autumnal crop. I was therefore led to try several different methods of culture, with a view to obtain plants that would just begin to blossom at the period when the other varieties cease; conceiving, that such plants, not having expended either themselves or the virtue of the soil in a previous crop of fruit, would afford the best and most abundant autumnal produce. Under this impression I sowed the seeds of

* Trans. of the Horticultural Soc. vol. I, p. 159.

the

the best alpine variety, that I had ever been able to obtain, in pots of mould, in the beginning of August, the seeds of the preceding year having been preserved to that period; and the plants these afforded were placed, in the end of March, in beds to produce fruit. Seeds sowed in August.

This experiment succeeded tolerably well; but I was not quite satisfied with it; for though my plants produced an abundant autumnal crop of fruit, they began to blossom somewhat earlier than I wished, and before they were perfectly well rooted in the soil. I therefore tried the experiment of sowing some seeds of the same variety early in the spring in pots, which I placed in a hotbed of moderate strength in the beginning of April, and the plants thus raised were removed to the beds in which they were to remain in the open ground, as soon as they had acquired a sufficient size. They began to blossom soon after Midsummer, and to ripen their fruit towards the end of July, affording a most abundant autumnal crop of very fine fruit; and even so late as the second week in December I have rarely seen a more abundant profusion of blossoms and immature fruit than the beds presented. The powers of life in plants thus raised, being young and energetic, operate much more powerfully than in the humours of older plants, or even in plants raised from seeds in the preceding year; and therefore I think the Alpine strawberry ought always to be treated as an annual plant. The plants blossom rather too early. Seeds sowed in spring yielded fruit very late. Should always be treated as an annual.

XI.

On the Nature of Heat. *By* MARSHALL HALL, *Esq.* *In a Letter from the Author.*

To W. NICHOLSON, Esq.

SIR,

THE nature of caloric has long been a subjeet of inquiry in chemical philosophy. The first conjecture on this matter, which deserves attention, is that of Lord Bacon; his opinion has, however, been in a great measure superseded by Nature of caloric long questioned. Hypothesis of Bacon.

: hypothesis of Homberg and Boerhaave. It may in-
ed be observed, that while the opinion of the materiality of caloric has had many adherents, and received much consideration, the hypothesis of Bacon has probably been too much neglected; nay, it has even been held up to ridicule and contempt, as a "delusive dream," or as a "labyrinth of perplexities." Probably the reason of this censure has been just given; had the opinion obtained the consideration which it merits, it would possibly have long since ceased to be this labyrinth of perplexity.

I trust therefore, that a few observations on this subject will not be altogether unwelcome: no apology can be required for their imperfections. I shall commence with a few remarks on the prevailing opinion, and shall then give a concise view of what may be termed the hypothesis of vibration.

1. *Sources of caloric.*

Sources of heat.

Scarcely any circumstance can afford more forcible objection to the hypothesis of material caloric, than some of the means of producing an increase of temperature. Every one is acquainted with the important researches of Bog!e, Romford, and Davy, on this subject. Their experiments prove, that heat may be produced by friction in circumstances, where no source of it, considered as a material agent, can be discovered or suspected.

But these facts have been so often urged as incompatible with the supposition of material caloric, that it is needless to enter farther into the discussion of this point. I wish rather to avail myself of this opportunity, to consider other parts of the doctrine.

Under this head it may indeed be added, that the excitation of heat, in the operations of electricity and galvanism, has not been explained. There is also much difficulty in accounting for the production of heat in some instances of combustion *, and of other chemical actions.

* Thomson, vol. I, p. 575 et seq. 3d ed.

2. *Motion*

2. *Motion of caloric.*

It is observed, that the best conductors of heat receive and deliver it most easily and rapidly; those bodies, which absorb heat with most avidity, are also such as radiate most copiously. The first part of these operations might be ascribed to the attraction exerted between the particles of the body and of caloric; but the second phenomena are directly adverse to such an explanation. Considering caloric as matter, and subject to attraction like other matter, the circumstances above related appear to require explanation. Motion of heat.

In the *radiation* of heat many phenomena occur, which have not been satisfactorily explained: and first, the remarkable difference between solar and culinary heat does not appear to be by any means understood *. The first is transmissible through and refrangible by glass, and other transparent media; the second is in a large proportion intercepted by every solid body: the first is reflected in circumstances, in which the second is absorbed †: culinary heat, unlike the solar ray, suffers a considerable aberration in its reflection: and lastly, the absorption of culinary heat is not affected by the colour of the absorbing surface, in the same manner as that of solar heat ‡. What can be the cause—what the rationale of these differences? Radiation of heat Difference between that of the sun & fire.

It is however in the radiation of *cold*, I conceive, that we have the most forcible and direct objection to the hypothesis of material caloric, and the most certain indication of the real nature of this principle. It is scarcely necessary to say, that no unexceptionable explanation of this phenomenon has been proposed. According to Prevost's supposition, the effect of radiation from a cold surface ought in reality to be that of heating, and not of cooling the opposed thermometer; this will be rendered evident by the assistance of a diagram. Radiation of cold.

Pl. VI, fig. 4, *a b*, *c d*, are two concave mirrors. T *a c* I, T *b d* I are rays of heat issuing from the thermometer, I *c a* T, I *d b* T, are rays *also of heat*, issuing from the ice; for,

* Nicholson's Journal, vol. viii, p. 297.

† Leslie's Inq. p. 83, et seq.

‡ Ibid, p. 97.

, according to the hypothesis, both the thermometer and ne ice radiate heat. The thermometer T however radiates the ice I more than the latter does to the thermometer; the ice therefore receives caloric, and will be dissolved; but the ice also radiates caloric; this will be reflected and conveyed to the thermometer, which will consequently maintain a higher temperature, than if there were no ice, from which it could receive caloric.

Count Rumford ascribes heat to undulations.

"Count Rumford, not admitting the existence of caloric as a distinct matter, endeavours to explain the phenomena of radiant heat from the hypothesis of undulations excited by bodies at a high temperature in an etherial medium."—The Stahlian theory accounted for the phenomena of oxidation, while philosophers neglected the agency of the atmospheric air in the operation; and in a similar manner the hypothesis of Count Rumford might explain the radiation of heat and cold, could we forget the manifest influence of the "*ambient air.*" Other difficulties in this hypothesis would occur, in applying it to explain the differences between solar and culinary heat; and in accounting for the partial interception and partial transmission of culinary heat by transparent media.

Difficulties in this hypothesis.

This partial transmission of culinary heat, and its distribution in the prismatic spectrum, do not appear to admit of explanation on the ingenious hypothesis of Mr. Leslie.

With regard to other attempts, which have been made to explain the radiation of cold, and to reconcile it to the general theory, complete satisfaction may be obtained from consulting Mr. Murray's work,

3. *Effects of caloric.*

Effects of heat.

The opinion respecting the mixture of material heat arises chiefly from the consideration of the effects, which the communication of temperature occasions on the bulk and form of bodies submitted to its action. The explanation, which the hypothesis affords, of the immediate effects of heat, is indeed often satisfactory; yet, although it applies in many cases, it fails altogether in others; and cannot, I conceive, bear the test of a strict examination.

Expands bodies generally,

1. If the hypothesis were true, expansion ought invariably

bly to attend an increase of temperature, and contraction ought constantly to accompany its diminution.

It is scarcely necessary to mention the contradictions to this law, observed in our operations on water, iron, and some saline solutions, while they retain their fluidity; on water, iron, bismuth, antimony, sulphur, the saline bodies, &c. during their transition from a solid to a fluid form; and on argil at high temperatures. but not always.

In the liquefaction of ice, iron, sulphur, &c., the contraction in bulk is very considerable; yet during the operation, from the temperature acquired, and especially from the increase of capacity, a very great quantity of caloric is supposed to be absorbed.

2dly, The degree of expansion ought, ceteris paribus, to be in direct proportion to the quantity of caloric absorbed. Expansion not in proportion to the heat supposed to be absorbed.

Now as in changes of temperature those bodies, which have the greatest capacity for caloric, absorb the greatest quantity, it follows, that their expansibility ought to be proportionate to their capacity. This is however by no means the case, as will be observed by the inspection of the following table, in which the expansibility and the capacity of several of the metals are compared.

	Capacity.	Expansibility.
Iron	·98982	100126
Copper	·98823	100170
Zinc	·64699	100296
Antimony	·43292	100109
Lead	·39959	100287

Of these metals, iron and lead occupy the extremes in capacity; iron having the largest and lead the least capacity for caloric; yet lead is the most, iron the least expansible by heat: that metal, therefore, which absorbs the most caloric, expands the least; and, on the other hand, that which absorbs the least of this repulsive fluid, expands the most! The same discrepancy is observed in other parts of the table; antimony and lead have a capacity nearly equal, yet they occupy the extremes, in the scale of expansibility.

Aware, however, that the expansibility of any body might be regulated altogether by the degree of cohesion between This apparently not owing to cohesion.

its

its particles, I examined the same circumstance in those bodies, the form of which precludes the operation of this case, at least to any very considerable extent. The following table will show the result.

	Capacity.	
Oxigen gas	5·238147	Expansibility equal.
Hidrogen gas	1·80402*	
Atmospheric air	1·79*	
Carbonic acid gas	1·5681	
Nitrogen gas	·78169	

All gasses then are found to expand in an equal degree by the same change in temperature; yet how widely different are the respective quantities of caloric absorbed! From the above table it appears, that, during a given increase of temperature, carbonic acid gas absorbs a quantity of caloric more than twice as great as the same bulk of nitrogen gas; atmospheric air, and hidrogen gas absorb a quantity still more considerable; and oxigen gas actually absorbs more than $6\frac{1}{2}$ times this quantity; and still the expansibility is precisely the same in all.

It is certainly needless to add any remarks on facts such as these; they are indeed truly important. One portion of caloric, the principle of repulsion in these operations, occasions an expansion in one case equal to that which $6\frac{1}{2}$ times this quantity does in another: this effect too takes place, when no cause occurs, to regulate or influence it.

4. *Capacity for Caloric.*

Capacity for heat.

The phenomena of the capacity of bodies for caloric appear to me, to be adverse to the opinion of its materiality. Caloric is the supposed cause of temperature and of expansion; yet we communicate caloric to ice, at 32° Fahr., without an increase of its temperature, and with an actual diminution of its bulk. Here then our material agent has forgotten its functions, and we are obliged to resort to a

* This statement from calculation agrees nearly with the results of Mr. Leslie's experiments on these two gasses. The same may be said of the oxigen and nitrogen gasses; $\frac{\cdot78169 \times 3 + 5\cdot238147}{4} = 1\cdot8958$ which, all circumstances considered, is wonderfully exact.

new hypothesis, to reconcile the contradiction: for I can regard the doctrine of capacity in no other light, than in that of a second hypothesis, adopted to obviate the imbecility of a former one.

The objections afforded by this part of the subject, to the general theory, are too palpable to need to be insisted on. If it be argued, that our notion of capacity supposes a power of counteracting the usual properties of caloric; as the properties of acid and alkali mutually neutralize each other; there are facts not less in contradiction to this supposition. Thus the caloric communicated to boiling water is expanded in satisfying its increased capacity; nevertheless the expansion occasioned is prodigiously great.

It has indeed been asserted, that there are *direct proofs* of the existence of material caloric; it is therefore proper, that we should consider these.

Communication of heat through a vacuum not proved.

1st, "The communication of caloric through a vacuum, has been regarded as such a proof." In opposition to this argument it is sufficient to state, that no absolute vacuum, as far as we know, has ever been effected. Cavallo could never render the sound of a bell even perfectly inaudible, although he employed an air pump of the best construction. And in the Torricellian vacuum it is well known, that an atmosphere of mercurial vapour is formed. Pictet has even observed the condensation of this vapour. By this vapour therefore may the heat be communicated, although not material; its tenuity affords no objection; the conducting power of bodies does not observe the ratio of their density.

Radiation of heat facilitated by the air.

It is observed, that the conducting power of the Torricellian vacuum is to that of the atmospheric air as 100 to 605. Now this presents a fact, which it is not easy to reconcile to the material theory. According to this theory, the radiating power of any body must depend on its own nature and power; it cannot be assisted, it may be opposed, by surrounding bodies; but the fact just stated, and the experiments of Mr. Leslie, prove, that radiation is in reality facilitated by the surrounding air.

Radiation of heat

2dly, "The radiation of caloric appears to be another unequivocal

...l proof of its materiality. A matter is thrown from ... bodies, which moves in right lines, with velocity, ...aises the temperature of any body, on which it falls; and which, in every state, preserves the properties of caloric."

no proof of its materiality.

But are these proofs of materiality? By no means. If every...hing were material, of which these properties could be p...licated, then should we have proofs of something substantial in *sound* and *cold*. Thus sound is thrown from surrou...g bodies, in right lines, with velocity, is capable of refl...on and of condensation, occasions sound in some bodies on which it falls, and, in every state, preserves the properties ...ou... ...so moves in right lines, with veloci... ...tion and condensation, lowers the tempe... ...and is always and absolutely cold.

Heat of the Sun distinct from light.

"Lastl... it ... that "the existence of caloric, in the rays of the sun, apart from visible light, adds to the proof, that a peculiar matter exists, possessed of the properties of caloric, and distinct from every other."

This will be considered hereafter.

It is sufficient to have mentioned this last alleged proof of the existence of material caloric; its validity rests entirely on the supposition, that no other explanation can be given of the phenomenon; and it will consequently fall to be considered, in the second division of our subject.

(To be concluded in our next.)

XII.

On some of the Combinations of Oximuriatic Gas and Oxigen, and on the Chemical Relations of these Principles to inflammable Bodies. By HUMPHRY DAVY, *Esq. LL. D. Sec. R. S. Prof. Chem. R. I. F. R. S. E.*

(Concluded from p. 127.)

3. *On the Combinations of the Metals of the Earths with Oxigen and Oximuriatic Gas.*

Muriates of the earths not

THE muriates of baryta, lime, and strontia, after being a long time in a white heat, are not decomposable by any simple

simple attractions: thus, they are not altered by boracic acid, though, when water is added to them, they readily afford muriatic acid and their peculiar earths. decomposable without water.

From this circumstance, I was induced to believe, that these three compounds consist merely of the peculiar metallic bases, which I have named barium, strontium, and calcium, and oximuriatic gas; and such experiments as I have been able to make, confirm the conclusion. Compounds of metals with oximuriatic gas.

When baryta, strontia, or lime, is heated in oximuriatic gas to redness, a body precisely the same as a dry muriate is formed, and oxigen is expelled from the earth. I have never been able to effect so complete a decomposition of these earths by oximuriatic gas, as to ascertain the quantity of oxigen produced from a given quantity of earth. But in three experiments made with great care I found, that one of oxigen was evolved for every two in volume of oximuriatic gas absorbed. The earths evolve one part of oxigen for two of oximuriatic gas.

I have not yet tried the experiment of acting upon oximuriatic gas by the bases of the alkaline earths; but I have not the least doubt, that these bodies would combine directly with that substance, and form dry muriates. Direct union not yet tried.

In the last experiment that I made on the metallization of the earths by amalgamation, I paid particular attention to the state of the products formed by exposing the residuum of amalgams to the air. I found, that baryta formed in this way was not fusible at an intense white heat, and that strontia and lime so formed gave off no water when ignited. Baryta made from crystals of the earth, as Mr. Berthollet has shown, is a fusible hydrate; and I found, that this earth gave moisture when decomposed by oximuriatic gas; and the lime, in hydrate of lime, was much more rapidly decomposed by oximuriatic gas than quicklime, its oxigen being rapidly expelled with the water. Earths produced from their metallic bases, not hydrates as the common earths.

Some dry quicklime was heated in a retort, filled with muriatic acid gas: water was instantly formed in great abundance, and it can hardly be doubted, that this arose from the hidrogen of the acid combining with the oxigen of the lime. Dry quicklime heated in muriatic gas.

As potassium so readily decomposes common salt, I thought it might possibly decompose muriate of lime, and Action of potassium on the

thus

muriates of the earths.

thus afford easy means of procuring calcium. The rapidity with which muriate of lime absorbs water, and the difficulty of freeing it even by a white heat from the last portions, rendered the circumstances of the experiments unfavourable. I found, however, that by heating potassium strongly, in contact with the salt, in a retort of difficultly fusible glass, I obtained a dark coloured matter, diffused through a vitreous mass, which effervesced strongly with water. The potassinm had all disappeared, and the retort had received a heat at which potassium entirely volatilizes. I had similar results with muriate of strontia, and (though less distinct, more potassium distilling off unaltered) with muriate of baryta. Either the bases of the earths were wholly or partially deprived of oximuriatic gas in these processess, or the potassium had entered into triple combination with the muriates. I hope on a future occasion to be able to decide this point.

Combination of magnesia, alumine, and silex, with muriatic gas.

Combinations of muriatic acid gas with magnesia, alumine, and silex, are all decomposed by heat, the acid being driven off, and the earth remaining free. I conjectured from this circumstance, that oximuriatic gas would not expel oxigen from these earths, and the suspicion was confirmed by experiments. I heated magnesia*, alumine, and silex to redness in oximuriatic gas, but no change took place.

Barytes absorbs oxigen.

Messrs. Gay-Lussac and Thenard have shown, that baryta is capable of absorbing oxigen; and it seems likely, (as, according to Mr. Chenevix's experiments, most of the earths are capable of becoming hyperoximuriates) that peroxides of their bases must exist.

Lime apparently not.

I endeavoured to combine lime with more oxigen, by heating it in hyperoximuriate of potash, but without success, at least after this process it gave off no oxigen in combining with water.

Oximuriate.

The salt, called oximuriate of lime, made for the use of the bleachers, I found gave off oxigen by heat, and formed muriate of lime.

Oxigen produced from oximuriatic gas and magnesia.

* From some experiments of Messrs Gay-Lussac and Thenard, Bullet. de la Societ. Phil. Mai, 1810, it appears, tha oxigen is rocured by passing oximuriatic gas over magnesia at a high temperature, and that a muriate indecomposable by heat is produced. They attribute the presence of this oxigen to the decomposition of the acid; but, according to all analogies, it must arise from the decomposition of the earth.

From

From the proportions which I have given in the last Bakerian lecture, but which were calculated from the analyses of sulphates, it follows, that, if the muriate of barytæ, strontia, and lime, be regarded as containing one proportion of oximuriatic gas, and one of metal, then they would consist of 71* barium, 46 strontium, and 21 calcium, to 32·9 of oximuriatic gas. Component parts of the earthy muriates.

To determine how far these numbers are accurate, 50 grains of each of these muriates, that had been heated to whiteness, were decomposed by nitrate of silver, the precipitate was collected, washed, heated, and weighed.

The muriate of baryta, treated in this way, afforded 68 grains of horn-silver.

The muriate of strontia 85 grains.

The muriate of lime 125 grains.

From experiments to be detailed in the next section, it appears, that horn-silver consists of 12 of silver to 3·9 of oximuriatic gas, and consequently, that barium should be represented by 65·1, strontium by 46·1, and calcium by 20·8. Horn silver.

4. *On the Combinations of the Common Metals with Oxigen and Oximuriatic Gas.*

In the limits which it is usual to adopt in this lecture, it will not be possible for me to give more than an outline of the numerous experiments, that I have made on the combinations of oximuriatic gas with metals; I must confine myself to a general statement of the mode of operating, a d the results. I used in all cases small retorts of green glass, containing from 3 to 6 cubical inches, furnished with stopcocks. The metallic substances were introduced, the retort exhausted and filled with the gas to be acted upon, heat was applied by means of a spirit lamp, and after cooling, the results were examined, and the residual gas analysed. Combinations of oximuriatic gas with metals.

All the metals that I tried, except silver, lead, nickel, cobalt, and gold, when heated, burnt in the oximuriatic gas, and the volatile metals with flame. Arsenic, anti- Metals heated in it.

* If Mr. James Thompson's analysis of sulphate of barytes be made the basis of calculation, sulphuric acid being estimated as 36, then the number representing barium will be about 65·5.

[illegible]my, tellurium, and zinc with a white flame, mercury with a red flame. Tin became ignited to whiteness, and iron and copper to redness; tungsten and manganese to dull redness; platina was scarcely acted upon at the heat of fusion of the glass.

Product from arsenic, The product from arsenic was butter of arsenic; a dense, limpid, highly volatile fluid, a nonconductor of electricity, and of high specific gravity, and which, when decomposed by water, gave oxide of arsenic and muriatic acid. That

antimony, from antimony was butter of antimony, an easily fusible and volatile solid, of the colour of horn-silver, of great density, crystallizing on cooling in hexaedral plates, and giving, by its decomposition by water, white oxide.

tellurium, The product from tellurium, in its sensible qualities, resembled that from antimony, and gave when acted on by water white oxide.

mercury, zinc, The product from mercury was corrosive sublimate. That from zinc was similar in colour to that from antimony, but was much less volatile.

iron, The combination of oximuriatic gas and iron was of a bright brown; but having a lustre approaching to the metallic, and was iridescent like the Elba iron ore. It volatilized at a moderate heat, filling the vessel with beautiful minute crystals of extraordinary splendour, and collecting in brilliant plates, the form of which I could not determine. When acted on by water, it gave red muriate of iron.

copper, Copper formed a bright red brown substance, fusible at a heat below redness, and becoming crystalline and semitransparent on cooling, and which gave a green fluid, and a green precipitate by the action of water*.

manganese, The substance from manganese was not volatile at a dull red heat; it was of a deep brown colour, and by the action of water became of a brighter brown: a muriate of manga-

* It is worth inquiry, whether the precipitate from oximuriate of copper by water is not a hydrated submuriate, analogous in its composition to the crystallized muriate of Peru. This last I find affords muriatic acid and water by heat.

Resin of copper. The resin of copper discovered by Boyle, formed by heating copper with corrosive sublimate, probably contains only 1 proportion of oximuriatic gas, while that above referred to must contain 2.

[illegible] nese,

nese, which did not redden litmus, remained in solution; and an insoluble matter remained of a chocolate colour*.

Tungsten afforded a deep orange sublimate, which, when decomposed by water, afforded muriatic acid, and the yellow oxide of tungsten. tungsten,

Tin afforded Libavius's liquor, which gave a muriate by the action of water containing the oxide of tin, at the maximum of oxidation. tin,

Silver and lead produced horn-silver and horn-lead, and bismuth, butter of bismuth. The absorption of oximuriatic gas was in the following proportions for two grains of each of the metals; for arsenic 3·6 cubical inches, for antimony 3·1, for tellurium 2·4, for mercury 1·05†, for zinc 3·2, for iron 5·8, for tin 4, for bismuth 1·5, for copper 3·4, for lead ·9; for silver, the absorption of volume was 0·9, and the increase of weight of the silver was equivalent to 0·6 of a grain‡. silver lead, and bismuth. Properties of gas absorbed.

In acting upon metallic oxides by oximuriatic gas, I found that those of lead, silver, tin, copper, antimony, bismuth, and tellurium, were decomposed in a heat below redness, but the oxides of the volatile metals more readily Action of oximuriatic gas on oxides.

* When muriate of manganese is made by solution of its oxide in muriatic acid, a neutral combination is obtained, but this is decomposed by heat; muriatic gas flies off, and brown oxide of manganese remains. In this respect manganese appears as a link between the ancient metals and the newly discovered ones. Its muriate is decomposed like that of magnesia; and its oxide is the only one amongst those long known, as far as my experiments have gone, which neutralizes the acid energy of muriatic acid gas, so as to prevent it in solution from affecting vegetable blues. Effect of oxide of manganese on muriatic acid.

† The gas in these experiments was not freed from aqueous vapour, and as stopcoks of brass were used, a little gas might have been absorbed by the surface of this metal, so that the processes offer only approximations to the composition of the oximuriates. The processes on lead, tellurium, iron, antimony, copper, tin, mercury, and arsenic, were carried on in three successive days, during which the height of the mercury in the barometer varied from 30·26 inches to 30·15, and the height of that in the thermometer from 63·5 to 61 Fahrenheit.

The experiment on silver was made at the temperature of 52 Fahrenheit, and under a pressure equal to that of 29·9 inches.

‡ This agrees nearly with another experiment made by my brother, Mr. John Davy, in which 12 grains of silver increased to 15·9 during their conversion into horn-silver.

 than

than those of the fixed ones. The oxides of cobalt and nickel were scarcely acted upon at a dull red heat. The red oxide of iron was not affected at a strong red heat, while the black oxide was rapidly decomposed at a much lower temperature; arsenical acid underwent no change at the greatest heat that could be given it in the glass retort, while the white oxide readily decomposed.

Oxigen given off as much as the metal absorbed.

In cases where oxigen was given off, it was found exactly the same in quantity as that which had been absorbed by the metal. Thus 2 grains of red oxide of mercury absorbed 0·9 of a cubical inch of oximuriatic gas, and afforded 0·45 of oxigen*. Two grains of dark olive oxide, from calomel decomposed by potash, absorbed about 0·94 of oximuriatic gas, and afforded 0·24 of oxigen, and corrosive sublimate was produced in both cases.

Analysis of corrosive sublimate and of calomel.

* I have made two analyses of corrosive sublimate and calomel, with considerable care. I decomposed 100 grains of corrosive sublimate by 90 grains of hydrat of potash. This afforded 79·5 grains of orange coloured oxide of mercury, 40 grains of which afforded 9·15 cubical inches of oxigen gas; the muriate of silver formed from the 100 grains was 102·5. 100 grains of calomel, decomposed by 90 grains of potash, afforded 82 grains of olive coloured oxide of mercury, of which 40 grains gave by decomposition by heat 4·8 cubical inches of oxigen. The quantity of horn-silver formed from the 100 grains was 58·75 grains.

In the second analysis, the quantity of oxide obtained from corrosive sublimate was 78·7; the quantity of muriate of silver formed was 103·4; the oxide produced from calomel weighed 83 grains; the horn-silver formed was 57½ grains. I am inclined to put most confidence in the last analyses; but the tenor of both is to show, that the quantity of oximuriatic gas in corrosive sublimate is exactly double that in calomel, and that the orange oxide contains twice as much oxigen as the black, the mercury being considered as the same in all. The olive colour of the oxide formed from calomel is owing to a slight admixture of orange oxide, formed by the oxigen of the water used in precipitation; the tint I find is almost black, when a boiling solution of potash is used; and trituration with a little orange oxide brings the tint to olive. It has been stated, that the olive oxide thrown down from calomel by potash is a submuriate; but I have never been able to find a vestige of muriatic acid in it when well washed. It is not easy to obtain perfect precision in analyses of the oxides of mercury; water adheres to the oxides, which cannot be entirely driven off without the expulsion of some oxigen. In all my experiments, though the oxides had been heated to a temperature above 212, a little dew collected in the neck of the retort, so that the 40 grains must have been overrated.

In

In the decomposition of the white oxide of zinc, oxigen was expelled exactly equal to half the volume of the oximuriatic acid absorbed. In the case of the decomposition of the black oxide of iron, and the white oxide of arsenic, the changes that occurred were of a very beautiful kind; no oxigen was given off in either case, but butter of arsenic, and arsenical acid formed in one instance, and the ferruginous sublimate, and red oxide of iron in the other. Oxides of zinc, iron, and arsenic.

Two grains of white oxide of arsenic absorbed 0·8 of oximuriatic gas*.

I doubt not that the same phenomena will be found to occur in other instances, in which the metal has comparatively a slight attraction only for oximuriatic gas, and when it is susceptible of different degrees of oxidation, and in which the peroxide is used.

The only instance in which I tried to decompose a common metallic oxide, by muriatic acid, was in that of the fawn coloured oxide of tin; a compound of water and Libavius's liquor separated. Oxide of tin.

From the proportions which may be gained in considering the volumes of oximuriatic gas absorbed by the different metals, in their relations to the quantity of oxigen which would be required to convert them into oxides, it would appear, that in the experiments to which I have referred, either one, two, or three proportions of oximuriatic gas combine with one of metal, and consequently, from the composition of the muriates, it will be easy to obtain the numbers representing the proportions in which these metals may be conceived to enter into other compounds†. One part of metal combines with one, two, or three of oximuriatic gas.

* A singular instance of the tendency of the oxide of arsenic to become arsenical acid occurs in its action on fused hydrat of potash, the water in the hydrat is rapidly decomposed, and arseniuretted hidrogen evolved, and arseniate of potash formed.

† From the experiments detailed in the note in the opposite page, it would appear that the number representing the proportion in which mercury combines must be about 200. That of silver, as would appear from the results, page 227, about 100. The numbers of other metals may be learnt from the data in the same page, but, from what has been stated, these data cannot be considered as very correct.

5. *General*

5. *General Conclusions and Observations, illustrated by Experiments.*

Former inferences confirmed.

All the conclusions, which I ventured to draw in my last communication to the Society, will, I trust, be found to be confirmed by the whole series of these new inquiries.

Oximuriatic gas combines with inflammable bodies.

Oximuriatic gas combines with inflammable bodies, to form simple binary compounds; and in these cases, when it acts upon oxides, it either produces the expulsion of their oxigen, or causes it to enter into new combinations.

The oxigen not from its decomposition.

If it be said, that the oxigen arises from the decomposition of the oximuriatic gas, and not from the oxides; it may be asked, why it is always the quantity contained in the oxide; and why in some cases, as those of the peroxides of potassium and sodium, it bears no relation to the quantity of gas.

No acid matter in it.

If there existed any acid matter in oximuriatic gas, combined with oxigen, it ought to be exhibited in the fluid compound of one proportion of phosphorus, and two of oximuriatic gas; for this, on such an assumption, should consist of muriatic acid (on the old hypothesis, free from water) and phosphorous acid; but this substance has no effect on litmus paper, and does not act, under common circumstances, on fixed alkaline bases, such as dry lime or magnesia. Oximuriatic gas, like oxigen, must be combined in large quantity with peculiar inflammable matter, to form acid matter. In its union with hidrogen, it instantly reddens the driest litmus paper, though a gaseous body. Contrary to acids, it expels oxigen from protoxides, and combines with peroxides.

Decomposition of potash by it.

When potassium is burnt in oximuriatic gas, a dry compound is obtained. If potassium combined with oxigen is employed, the whole of the oxigen is expelled, and the same compound formed. It is contrary to sound logic to say, that this exact quantity of oxigen is given off from a body not known to be compound, when we are certain of its existence in another; and all the cases are parallel.

Production of oximuriatic gas from muriatic & oxide of manganese.

An argument in favour of the existence of oxigen in oximuriatic gas may be derived by some persons from the circumstances of its formation, by the action of muriatic acid

on

on peroxides, or on hyperoximuriate of potash; but a minute investigation of the subject will, I doubt not, show, that the phenomena of this action are entirely consistent with the views I have brought forward. By heating muriatic acid gas in contact with dry peroxide of manganese, water I found was rapidly formed, and oximuriatic gas produced, and the peroxide rendered brown. Now as muriatic acid gas is known to consist of oximuriatic gas and hidrogen, there is no simple explanation of the result, except by saying, that the hidrogen of the muriatic acid combined with oxigen from the peroxide to produce water.

Scheele explained the bleaching powers of the oximuriatic gas by supposing, that it destroyed colours by combining with phlogiston. Berthollet considered it as acting by supplying oxigen. I have made an experiment, which seems to prove, that the pure gas is incapable of altering vegetable colours; and that its operation in bleaching depends entirely upon its property of decomposing water, and liberating its oxigen.

Its bleaching effect

owing to the decomposition of water.

I filled a glass globe, containing dry powdered muriate of lime, with oximuriatic gas. I introduced some dry paper tinged with litmus, that had been just heated, into another globe containing dry muriate of lime; after some time this globe was exhausted, and then connected with the globe containing the oximuriatic gas, and by an appropriate set of stopcocks, the paper was exposed to the action of the gas. No change of colour took place, and after two days there was scarcely a perceptible alteration.

Litmus paper not affected by dry oximuriatic gas,

Some similar paper dried, introduced into gas that had not been exposed to muriate of lime, was instantly rendered white*.

whitened by moist.

Paper that had not been previously dried, brought into contact with dried gas, underwent the same change, but more slowly.

Moist paper changed more slowly.

The hyperoximuriates seem to owe their bleaching powers entirely to their loosely combined oxigen; there is a strong

Hyperoximuriates act by

* The last experiments were made in the laboratory of the Dublin Society; most of the preceding ones in the laboratory of the Royal Institution; and I have been permitted to refer to them by the Managers of that useful public establishment.

tendency

Loosely combined oxigen.

tendency in the metal of those in common use, to form simple combinations with oximuriatic gas, and the oxigen is easily expelled or attracted from them.

Oximuriatic gas not condensed and crystallized by cold.

It is generally stated in chemical books, that oximuriatic gas is capable of being condensed and crystallised at a low temperature; I have found by several experiments, that this is not the case. The solution of oximuriatic gas in water freezes more readily than pure water, but the pure gas dried by muriate of lime undergoes no change whatever, at a temperature of 40 below 0° of Fahrenheit. The mistake seems to have arisen from the exposure of the gas to cold in bottles containing moisture.

Boracium, phosphorus, iron, and arsenic, attract oxigen more strongly;

I attempted to decompose boracic and phosphoric acids by oximuriatic gas, but without success: from which it seems probable, that the attractions of boracium and phosphorus for oxigen are stronger than for oximuriatic gas. And from the experiments I have already detailed, iron and arsenic are analogous in this respect, and probably some other metals.

some other substances oximuriatic gas.

Potassium, sodium, calcium, strontium, barium, zinc, mercury, tin, lead, and probably silver, antimony, and gold, seem to have a stronger attraction for oximuriatic gas than for oxigen.

Combinations of oximuriatic compounds.

I have as yet been able to make very few experiments on the combinations of the oximuriatic compounds with each other, or with oxides. The liquor from arsenic, and that from tin, mix, producing an increase of temperature; and the phosphuretted, and the sulphuretted liquors unite with each other, and with the liquor of Libavius, but without any remarkable phenomena.

Oximuriates of phosphorus and lime.

I heated lime gently in a green glass tube, and passed the phosphoric sublimate, the saturated oximuriate of phosphorus through it, in vapour; there was a violent action with the production of heat and light, and a gray fused mass was formed, which afforded, by the action of water, muriate and phosphate of lime,

Indications of

I introduced some vapour from the heated phosphoric sublimate into an exhausted retort containing dry paper tinged with litmus; the colour slowly changed to pale red. This fact seems in favour of the idea, that the substance is an

[b]ut as some minute quantity of aqueous vapour [mig]ht have been present in the receiver, the experiment [cannot be] regarded as decisive; the strength of its attraction [for ammon]ia is perhaps likewise in favour of this opinion. [The ox]imuriates that I have tried, indeed, form triple [compound]s with this alkali; but the phosphorus is expelled [by th]e heat from the other compounds of oximuriatic [gas and p]hosphorus with ammonia, and the substance re[maining i]n combination is the phosphoric sublimate. its acidity.

Reflections on the Nomenclature of the Oximuriatic Compounds.

[To call] a body which is not known to contain oxigen, and [which can]not contain muriatic acid, oximuriatic acid, is [contrary t]o the principles of that nomenclature in which it [is adopted]; and an alteration of it seems necessary to assist [the progre]ss of discussion, and to diffuse just ideas on the [subject.] If the great discoverer of this substance had signi[fied it by] any simple name, it would have been proper to [have refe]rred to it; but dephlogisticated marine acid is a [term whic]h can hardly be adopted in the present advanced [state of] science. Nomenclature of oximuriatic gas.

[After co]nsulting some of the most eminent chemical phi[losophers] in this country, it has been judged most proper [to propose] a name founded upon one of its obvious and cha[racteristic] properties—its colour, and to call it *chlorine*, or [chloric gas]*. Chlorine, or chloric gas.

[Should] it hereafter be discovered to be a compound, and [even con]tain oxigen, this name can imply no errour, and [cannot ne]cessarily require a change.

[Most of] the salts, which have been called muriates, are not [known to] contain any muriatic acid, or any oxigen. Thus [Libavius's] liquor, though converted into a muriate by water, [contains o]nly tin and oximuriatic gas, and horn-silver seems [incapable] of being converted into a true muriate. Salts improperly called muriates.

[I ventur]e to propose for the compounds of oximuriatic [gas and i]nflammable matter the name of their bases, with [the termin]ation *ane*. Thus argentane may signify horn- Compounds of oximuriatic gas.

* From χλωρος.

silver

silver; stannane, Libavius's liquor; antimonane, butter of antimony; sulphurane, Dr. Thomson's sulphuretted liquor; and so on for the rest.

In cases when the proportion is one quantity of oximuriatic gas and one of inflammable matter, this nomenclature will be competent to express the class to which the body belongs, and its constitution. In cases when two or more proportions of inflammable matter combine with one of gas; or two or more of gas, with one of inflammable matter, it may be convenient to signify the proportions by affixing vowels before the name, when the inflammable matter predominates, and after the name, when the gas is in excess; and in the order of the alphabet, *a* signifying two, *e* three, *i* four, and so on.

Muriates. The name muriatic acid, as applied to the compound of hidrogen and oximuriatic gas, there seems to be no reason for altering. And the compounds of this body with oxides should be characterised in the usual manner, and as the other neutral salts.

Thus muriate of ammonia and muriate of magnesia are perfectly correct expressions.

I shall not dwell any longer at present upon this subject. —What I have advanced, I advance merely as suggestion, and principally for the purpose of calling the attention of philosophers to it*. As chemistry improves, many other alterations

* It may be conceived, that a name may be found for oximuriatic gas in some modification of its present appellation, which may harmonize with the new views, and which may yet signify its relation to the muriatic acid, such as demuriatic gas, or oximuric gas; but in this case it would be necessary to call the muriatic acid, hydrogenated muriatic acid, or hydromuriatic acid; and the salts which contain it hydrogenated muriates or hydromuriates; and on such a plan, the compounds of oximuriatic gas must be called demuriates or oximuriates, which I conceive would create more complexity and difficulty in unfolding just ideas on this department of chemical knowledge, than the methods which I have ventured to propose. It may however be right, considering the infant state of the investigation, to suspend for a time the adoption of any new terms for these compounds. It is possible, that oximuriatic gas may be compound, and that this body and oxigen may contain some common principle; but at present we have no more right to say that oximuriatic gas contains oxigen, than to say that tin contains hidrogen; and names should

us will be necessary; and it is to be hoped, that, they take place, they will be made independent

press things, and not opinions; and till a body is decomhould be considered as simple.

ast number of Mr. Nicholson's Journal, which appeared st, while this sheet was correcting for the press, I have seen us paper, by Mr. Murray, of Edinburgh, in which he has to show, that oximuriatic gas contains oxigen. His methods tonating oximuriatic gas in excess with a mixture of hid gaseous oxide of carbon, when he *supposes* carbonic acid and by mixing oximuriatic gas in excess with sulphuretted when he *supposes* sulphuric acid, or sulphureous acid is n some experiments, in which my brother, Mr. John Davy, d as to cooperate, made over boiled mercury, we found, s of hidrogen, 8 parts of gaseous oxide of carbon, and 20 imuriatic gas, exploded by the electric spark, diminished measures; and calomel was formed on the sides of the tube. dry ammonia in excess, and exposing the remainder to remained, which equalled more than 9 measures, and which s oxide of carbon, with no more impurity than might be m the air in the gasses, and the nitrogen expelled from the so that the oxigen in Mr. Murray's carbonic acid, it seems, d from *water*, or from the carbonic oxide. Sulphuretted dded, over dry mercury, to oximuriatic gas in excess, intwo or three experiments; muriatic acid gas, containing of oximuriate of sulphur, was formed, which, when neutralnonia, gave muriate of ammonia, and a combination of amoximuriate of sulphur.

Supposed decomposition of oximuriatic gas.

ixture of oximuriatic gas in excess, and sulphuretted hidroffered to pass into the atmosphere, the smell was that of of sulphur; there was not the slightest indication of the any sulphuric or sulphureous acid. If Mr. Murray had nia, instead of water, for analyzing his results, I do not ould have concluded, that oximuriatic gas is capable of on by such methods.

ot, at present, enter upon a detail of other experiments, e made on this subject, in cooperation with my brother, as ation to refer to them, in an answer to Mr. Murray's paper. nclude, by saying, that this ingenious chemist has mistaken supposing them hypothetical; I merely state what I have

of all speculative views, that new names will be derived from some simple and invariable property, and that mere arbitrary designations will be employed, to signify the class to which compounds or simple bodies belong.

SCIENTIFIC NEWS.

Wernerian Natural History Society.

Iceland crystal. AT the meeting of this Society on the 27th of April, Professor Jameson read a paper concerning the geognostic relations of the Iceland doubly-refracting crystal. The secretary communicated an account of the habits of the colymbus immer, or ember-goose, by Dr. Edmonston of Lerwick. And Dr. Gordon read an interesting paper, consisting of observations and experiments on the qualities of sensation of sound; on the different modes in which sonorous vibrations are communicated to the auditory nerve; on the ideas of the distance, and of the angular position of sounding bodies with respect to the ear, which are associated, by experience, with the different qualities of sounds; and on some of the more remarkable differences in the *sense of hearing*, both original and accidental, which are occasionally observed among individuals, and, in particular, on the musical ear.

Ember-goose.

Qualities of sound.

Report of the Proceedings of the Mathematical and Physical Class of the French Institute, continued from p. 159.

Albumen of seeds affords nutriment to the plant.

Mr. Mirbel has continued his researches into the physiology of plants. Hitherto it had been acknowledged indeed, that the albumen of seeds commonly served to nourish the young plant after germination; but this opinion required the support of positive observations, and Mr. Mirbel appears to have removed all doubts respecting it by an experiment as simple as ingenious. The embryo in the seed of the onion bends as it unfolds itself, so as to form an elbow that rises out of the ground, while the plumula and radicle remain concealed in it. If at this stage of vegetation a mark

mark of any kind be made at equal heights on the two branches of the germe, the mark nearest the radicle will rise alone, if the plant received no aliment but from the juices of the Earth; on the contrary, if it were nourished solely by the albumen of the seed, the mark on the plumule would rise above the other: and lastly, the marks would rise pretty equally, if both the ground and the seed concurred in the developement of the germe. It is the latter phenomenon that takes place; and it ceases when the albumen is entirely absorbed: the young plant has then strength enough, to derive from the ground or the atmosphere the nourishment it thenceforward requires.

This paper is accompanied with interesting observations on the germination of asparagus, and on the manner in which the leaves of this plant, at first ensheathed like all those of the monocotyledons, become, by the growth of the stalk, lateral and opposite, and afterward lateral and alternate. Germination of asparagus,

In another paper Mr. Mirbel has examined the germination of the nelumbium. Botanists were not unanimous respecting the class, to which this plant should be referred, or the nature of the two fleshy lobes, from between which it springs. Some, observing no radicles developed in the germination of this plant, suppose it to be destitute of them: some consider these lobes as roots; others as peculiar organs analogous to the vitellus. Mr. M. has endeavoured to remove these doubts by his dissections. In the first place he finds in the nelumbium all the characters of a plant with more than one cotyledon; he next finds in the lobes vessels analogous to those of cotyledons; and at the juncture of the lobes he observes other vessels, uniting in the same manner as those that are characteristic of the radicles in embryos furnished with them. Hence he concludes, that the water lily does not differ essentially from the other plants of its class. and of the water lily.

Mr. Correa, while he agrees with Mr. M. in considering the nelumbium as a dicotyledon, differs from him on the nature of the lobes. He thinks, with Gaertner, that they have a great analogy to the vitellus, and he compares them with the fleshy tubercles of the roots of orchis. Plants he observes The lobes of the water lily analogous to the vitellus.

observes have a double organization, relating, on the one hand, to the earth, in which they spread their roots; on the other, to the air, in which their leaves are expanded. The roots are destined to the ascending vegetation; the leaves, to the descending; and it is at the point where these two systems unite, that the cotyledons are usually placed. But the lobes of the nelumbium are at the lowest part of the plant, and consequently belong to the roots. The example of many other plants destitute of cotyledons shows, that they are not essential to vegetation; and that the characters derived from them to arrange the vegetable kingdom in three divisions are insufficient, and should be replaced by those arising from the direction of the vessels and medullary radii.

Germination of grasses.

Mr. Poiteau has examined the germination of grasses. The part of the seed, which ought to be considered as the cotyledon, is yet questioned among botanists. Mr. P., observing that the scutum, which Gaertner took for a vitellus, and Mr. Richard for the body of the radicle, was placed at the point where the plumula and radicle separate, deems this a true cotyledon. Mr. P. has observed too, that, the moment when the radicle of a grass is unfolded, it assumes the figure of a cone, and represents the taproot of other plants; but as soon as the lateral roots have acquired a certain growth, this cone is obliterated, so that no plant of this family has a taproot. And as Mr. P. has made the same observation on several other monocotyledons, this substitution of numerous secondary roots for one principal root takes place, because each bundle of fibres of the monocotyledons has its peculiar root.

Amphibious mammalia.

The researches of Mr. Cuvier concerning fossil animals have commonly led him to discussions respecting the species admitted by naturalists, tending generally to the advancement of the science of zoology. Thus in considering the organization of the amphibious mammaliæ, he has been led to separate from the seals and morses, the Indian wallrus, the manatees, and the species described by Steller. These three genera form one family, distinguished by the absence of the posterior extremities, and by herbivorous teeth.

In

In another paper, on the genus felis, he gives the osteological characters of the head in the principal species; and has made known a species not distinguished by modern naturalists. To this he has given the name of leopard, which had become synonimous with panther, for want of being able to apply it with precision. It differs from the latter in being of a smaller size, and having a greater number of spots. Feline genus. Leopard.

Mr. Geoffroy long ago made a particular division, under the name of ateles, of the apes without thumbs, which had been confounded with the sapajous, from the prehensile tail common to both. He has now added two new species to those he had already made known, and given figures and descriptions of them. One of them, which he names arachnoides, had been merely mentioned by Edwards and Brown. The other, which he terms *encadreé* is altogether new. It is black, with white hairs round the face. Classification of apes. Two new species.

The same gentleman has described two birds; one imperfectly known, the other new. The latter has some resemblance both to the corvus nudus and the c. calvus; but there are sufficient differences between them to form three distinct genera, under the names of cephalopterus for the new species, gymnoderus for the c. nudus, and gymnocephalus for the c. calvus. Ornithology.

The cephalopterus is black, with a very high crest, which falls forward on the beak, and a kind of dewlap also covered with feathers. Each of these is of a metallic violet colour. Cephalopterus.

The other bird, which had beeu imperfectly described by Marcgrave under the name of cariama, Mr. G. had considered from his description as approaching to the trumpeter; but now he has seen it in the Museum of Natural History he classes it as a separate genus under the name of microdactylus. Microdactylus.

The tortoises have furnished Mr. G. with the subject of another interesting paper. Having seen, while in Egypt, the tortoise of the Nile, mentioned by Forskaol, he was led to form a particular genus of all those, which like it have the extremities of the ribs separate, and a soft shell. He names it trionix, and has added to it several new species. Tortoises.

Mr. Brougniart, in his elegant general treatise on reptiles, had

had classed these with his emydes; noticing at the same time the characters, that distinguish them from all the other species, the shell of which is complete and hard. The large softshelled tortoise of Bartram Mr. G. places in the genus chelys of Duméril.

Monography of tortoises.

Twenty years ago scarcely thirty species of tortoises were known, but nearly twice as many are accurately described by Mr. Sweiger in his general monography of tortoises. In this work a copious list of synonimes is given, and it is illustrated with figures carefully engraved.

Icthyology.

The class of fishes too has been enriched with many new species. Mr. Risseau and Mr. Delaroche have communicated the observations they made on this subject, the former in the Gulf of Nice, the latter in the sea round the Balearic Islands. It has been supposed, that fishes had their peculiar climates, but Mr. R. has found in the Mediterranean fishes considered as peculiar to the East Indies, and others known only in the northern seas. Mr. Delaroche made some interesting observations on the depth at which different fishes habitually live, the manner of catching them, and their airbladders.

Respiration of the crocodile.

Notwithstanding the difficulty of physiological experiments, and the nicety required in them, Mr. von Humboldt made many during his dangerous and toilsome travels. He has communicated his experiments on the respiration of the sharpnosed crocodile of America. He found, that, notwithstanding the volume of its bronchiæ, and the structure of its pulmonary cells, it suffers greatly without a supply of fresh air; its breathing is very slow; and a young one a foot long deprived the air of scarcely 12 cubic inches of oxigen in an hour and forty three minutes.

To be concluded in our next.

An accident has rendered us unable to insert our usual Meteorological Journal this month.

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

AUGUST, 1811.

ARTICLE I.

On the Motion of Rockets both in Nonresisting and Resisting Mediums. By W. MOORE, *Esq.*

(Continued from Vol. XXVIII, *p.* 169.*)*

To Mr. W. NICHOLSON.

SIR,

THE following is a farther extension of my essay concerning the motion of rockets in different mediums; which, if worthy acceptance, is quite at your service. The last two propositions are given as preparatory to my next inquiry, which is that of the several effects of the wind upon the first motion of these machines, when it is blowing in any given direction and velocity; which I will communicate to you as soon as time will allow me properly to prepare a paper of them.

From the results of the propositions that here follow, some very curious and important facts are ascertained; as that the motion of a *rocket* can never become uniform throughout the time of its burning under any law of resistance whatever; that bodies projected into resisting me- Curious facts ascertained in the following propositions.

diums cannot, independent of gravity, describe certain finite spaces but in infinite times, let the velocity of projection be what it may, great or small; the ratio of the resistance of a sphere to that of its circumscribing cylinder, when this moves in a direction perpendicular to its axis; and many other very curious particulars, as the person who shall read this paper will find. The investigations of the resistance to cylinders moving in fluids in directions different from that of their axes are new, as far as I know. No work, that I am acquainted with, contains a solution to this problem generally, but merely of the common particular case, where the solid is supposed to move in the direction of its axis; and perhaps the flight of rockets is one out of but very few cases in which the subject is at all applicable.

With thanks for the attention which you have hitherto paid to my communications, and respect for that impartiality and ability, with which your Journal is conducted.

I am, Sir,

Your most obedient servant,

Royal Academy, W. MOORE.
June 1811.

PROP. 6.

The motion of a rocket can never become uniform.

To determine whether the Motion of a Rocket ascending vertically in the Atmosphere can ever become uniform: the law of resistance being directly as the square of the velocity, as before.

When the motion of a body becomes uniform, or the velocity a maximum, the accelerative force is then nothing: therefore putting $\frac{(sned^2 b^2 - R v^2) a}{(am - ct) . b^2} - 1$ the accelerative force (see the last Prop.) $= o$, and reducing the equation, we have $v = \pm b . \left(\frac{sned^2 a - am + ct}{R a}\right)^{\frac{1}{2}}$. Whence it appears, that the velocity, and consequently the motion of the rocket can never become equable; being in terms of t the time of its burning; but will be greater and greater until the end of the time t, when the velocity will continually decrease till the whole is destroyed by the retardive force of gravity.

·. And it is moreover evident, that the motion of ...et can never become uniform under any law of resistance whatever.

Prop. 7.

...ings remaining as in the 5th Proposition: to find the ...city and Space described by the Rocket, when it is in...eced only by the impelling Force of the Composition and Resistance of the Medium.

Velocity and space described by a rocket from the impulse of its composition and resistance of the medium.

...e, gravity not acting, the accelerative force of the ... at the end of the time t will be $\frac{(sned^2 b^2 - Rv^2)a}{(am - ct)b^2}$ ...termined in Prop. 6. Therefore $\dot{v} = 2gf\dot{t} = \frac{(...d^2 b^2 - Rv^2).2ag\dot{t}}{(am - ct).b^2}$ = (putting $h = 2agsned^2 b^2$, ... agR, $l = amb^2$, and $p = cb^2$) $\frac{h\dot{t} - kv^2\dot{t}}{l - pt}$; and

$\frac{\dot{v}}{h - kv^2} = \frac{\dot{t}}{l - pt}$, whereof the fluent is $\frac{1}{2.(hk)^{\frac{1}{2}}}$. hyp. log.

$$\frac{(\ldots)^{\frac{1}{2}} + v}{(\ldots)^{\frac{1}{2}} - v} = -\frac{1}{p}.\text{ hyp. log. }\left(\frac{l}{p} - t\right)$$ which, when ... and $t = 0$, is $0 = -\frac{1}{p}$. hyp. log. $\frac{l}{p}$: therefore the ...t fluent is

$$\frac{1}{2(hk)^{\frac{1}{2}}}.\text{ hyp. log. }\frac{\left(\frac{h}{k}\right)^{\frac{1}{2}} + v}{\left(\frac{h}{k}\right)^{\frac{1}{2}} - v} = \frac{1}{p}$$

...p. log. $\frac{l}{p}$ − hyp. log. $\left(\frac{l}{p} - t\right)\Big\} = \frac{1}{p}$. hyp. log. $\frac{\ldots}{\ldots t}$: and hence by the nature of logs.

Velocity and space described by a rocket from the impulse of its composition and resistance of the medium.

$$\left(\frac{\left(\frac{h}{k}\right)^{\frac{1}{2}}+v}{\left(\frac{h}{k}\right)^{\frac{1}{2}}-v}\right)^{\frac{p}{2\,(hk)^{\frac{1}{2}}}} = \frac{l}{l-pt} \text{ : or,}$$

$\left(\frac{h}{k}\right)^{\frac{1}{2}} = j$ and $\frac{(hk)^{\frac{1}{2}}}{p} = w$, we shal

$$\frac{j+v}{j-v} = \frac{l^{w}}{(l-pt)^{w}}$$; and by reducing this

$$= \frac{jl^{w} - j\,(l-pt)^{w}}{l^{w} + (l-pt)^{w}}$$; which, when $t = a$,

$$\frac{jl^{w} - j\,(l-pa)^{w}}{l^{w} + (l-pt)^{w}}$$ the velocity of the rocket whe ceases burning. Or, restoring the values of j, w, l, the velocity of the rocket in thiscase will be expre

$$db.\left(\frac{sne}{R}\right)^{\frac{1}{2}}.\left\{\frac{(amb^{2})^{\frac{4agd\,(sneR)^{\frac{1}{2}}}{cb}} - (amb^{2}-acb^{2})^{\frac{4agd\,(s}{cb}}}{(amb^{2})^{\frac{4agd\,(sneR)^{\frac{1}{2}}}{cb}} + (amb^{2}-acb^{2})^{\frac{4agd\,(sneR}{cb}}}\right.$$

Now to determine what this velocity is, we must fi the value of R for the given case of velocity b. Now the conditions, that the particles of the medium are p nonelastic, and that the medium is infinitely com and affords no resistance to the motion of the rocket bu arises from the inertia of its particles, (which is the of our hypotheses concerning the law of resistance), w putting r for the radius of the rocket's base or of t of the rocket; $f =$ the sine of the angle, which the side of the head, (supposing it conical) makes with th $p = 3{\cdot}1416$; S = the specific gravity of the m which is here considered as the atmosphere; and $g =$ (omitting the $\frac{1}{12}$) have $R = \frac{pSr^{2}b^{2}f^{2}}{4g}$ (investig most works of fluxions and mechanics).

Let $b = 1$, in order to render the expression as sin

P

possible; and the angle, the sine of which is f, 30 degrees; then $f = \cdot 5$ or $\frac{1}{2}$ (to rad. 1): and taking the specific gravity of air at a medium, or $S = 1\frac{1}{4}$, R will be found $= \cdot 0002343$ ounces; which is the absolute resistance the rocket suffers when moving with a velocity of 1 foot per second. Hence the expression above for v will become - - - - - - - -

$$\frac{d\left(\frac{snc}{\cdot 0002343}\right)^{\frac{1}{2}} \left\{ (am)^{\frac{4agd}{c}(\cdot 0002343\, sne)^{\frac{1}{2}}} - (am - ac)^{\frac{4agd}{c}(\cdot 0002343\, sne)^{\frac{1}{2}}} \right\}}{(am)^{\frac{4agd}{c}(\cdot 0002343\, sne)^{\frac{1}{2}}} + (am - ac)^{\frac{4agd}{c}(\cdot 0002343\, sne)^{\frac{1}{2}}}}$$

and substituting the values for a, c, d, &c., which are as follow: namely,

$$\begin{aligned}
s &= 1000 \\
n &= 230 \text{ ozs.} \\
w &= 18 \text{ lbs.} = 288 \text{ ozs.} \\
c &= 10 \text{ lbs.} = 160 \text{ ozs.} \\
m &= w + c = 448 \text{ ozs.} \\
a &= 3 \text{ sec.} \\
d &= \tfrac{1}{4} \text{ ft.} \\
g &= 16 \text{ ft.} \\
e &= \cdot 7854
\end{aligned}$$

it is $v = \dfrac{6941\cdot 575 \left(1344^{1\cdot 95171} - 864^{1\cdot 95171}\right)}{1344^{1\cdot 95171} + 864^{1\cdot 95171}}$

$= \dfrac{6941\cdot 575 \times 737094}{1814186} = 2820\cdot 325$ feet: which is therefore the greatest velocity the rocket can acquire, and which it does acquire at the end of its burning. Velocity.

It is somewhat remarkable, that the whole resistance of the air to the rocket, on the supposition that gravity does not act, should so nearly approximate to the effect of this force (considered as constant) when there is no consideration of any resistance from the former; the deviation causing no more than ($2896\cdot 9895 - 2820\cdot 325 =$) $76\cdot 6645$ feet per second

Velocity. second difference in the greatest velocity of the rocket on the side of gravity.

To find the space described. By the theory of variable motions $\dot{x} = v\dot{t} = \frac{jl^w\dot{t} - j\dot{t}\,(l-pt)^w}{l^w + (l-pt)^w} = j\dot{t} - \frac{2j\dot{t}\,(l-pt)^w}{l^w+(l-pt)^w}$. Put $l - pt = \mathrm{T}$; then $\dot{\mathrm{T}} = -p\dot{t}$, and $\dot{t} = \frac{-\dot{\mathrm{T}}}{p}$. Whence $\dot{x} = \frac{-j\dot{\mathrm{T}}}{p} + \frac{2j}{p}\cdot\frac{\mathrm{T}^w\dot{\mathrm{T}}}{l^w+\mathrm{T}^w}$ $=$ (by expanding $\frac{\mathrm{T}^w\dot{\mathrm{T}}}{l^w+\mathrm{T}^w}$ in a series) $-\frac{j\dot{\mathrm{T}}}{p} + \frac{2j}{p}$ $-\left(\frac{\mathrm{T}^w\dot{\mathrm{T}}}{l^w} - \frac{\mathrm{T}^{2w}\dot{\mathrm{T}}}{l^{2w}} + \frac{\mathrm{T}^{3w}\dot{\mathrm{T}}}{l^{3w}} - \frac{\mathrm{T}^{4w}\dot{\mathrm{T}}}{l^{4w}} + \&c.\right)$; the fluent of which is $x = \frac{-j\mathrm{T}}{p} + \frac{2j}{p}\left(\frac{\mathrm{T}^{w+1}}{(w+1)\,l^w} - \frac{\mathrm{T}^{2w+1}}{(2w+1).l^{2w}} + \frac{\mathrm{T}^{3w+1}}{(3w+1)\,l^{3w}} - \frac{\mathrm{T}^{4w+1}}{(4w+1).l^{4w}} + \&c.\right)$ $= \frac{-j\mathrm{T}}{p} + \frac{2j\mathrm{T}^{w+1}}{pl^w}\cdot\left(\frac{1}{w+1} - \frac{\mathrm{T}^w}{(2w+1)\,l^w} + \frac{\mathrm{T}^{2w}}{(3w+1)\,l^{2w}} - \frac{\mathrm{T}^{3w}}{(3w+1)\,l^{3w}} + \&c.\right) = \frac{j}{p}\cdot$ $\left\{-(l-pt) + \frac{2(l-pt)^{w+1}}{l^w}\left(\frac{1}{w+1} - \frac{(l-pt)^w}{(2w+1).l^w} + \frac{(l-pt)^{2w}}{(3w+1)\,l^{2w}} - \frac{(l-pt)^{3w}}{(4w+1).l^{3w}} + \&c.\right)\right\}$; and the fluent corrected $x = \frac{j}{p}\left\{l - 2l.\left(\frac{1}{w+1} - \frac{1}{2w+1} + \frac{1}{3w+1} - \frac{1}{4w+1} + \&c.\right)\right\} + \frac{j}{p}\cdot$ $\left\{-(l-pt) + \frac{2(l-pt)^{w+1}}{l^w}.\left(\frac{1}{w+1} - \frac{(l-pt)^w}{(2w+1).l^w} + \frac{(l-pt)^{2w}}{(3w+1)\,l^{2w}} - \frac{(l-pt)^{3w}}{(4w+1).l^{3w}} + \right.\right.$

$$\left.\left.\&c.\right)\right\} = (\text{when } t = a)\ j. \left\{ a + \frac{2\,(l - ap)^{w}}{l^{w}}. \right.$$

$$\left(\frac{1}{w+1} - \frac{(l - ap)^{w}}{(2w+1).l^{w}} + \frac{(l - ap)^{2w}}{(3w+1).l^{2w}} - \right.$$

$$\left.\frac{(l - ap)^{3w}}{(3w+1).l^{3w}} + \&c.\right) - \frac{2\,l}{p}. \left(\frac{1}{w+1} - \frac{1}{2w+1}\right.$$

$$\left.\left. + \frac{1}{3w+1} - \&c.\right)\right\}$$ for the space described by the rocket at the end of the time t.

Now to determine how far the rocket will farther move before its motion is wholly destroyed. Put a = the velocity at the end of its burning = 2820·325 feet per second, and v any variable velocity corresponding to the space x; w = weight of the rocket = 448 ozs., and R = ·0002343 ounces, the resistance of the medium to the rocket when moving with a velocity of 1 foot per second. Then Rv^2 will be the resistance to velocity v, and $\frac{Rv^2}{w}$ the force by which the rocket is retarded by the fluid. Hence $\dot{x} = \frac{-v\dot{v}}{2fg} = -\frac{w\dot{v}}{2gRv}$, and $x = \frac{-w}{2gR}.$ hyp. log. v; and the fluent corrected $x = \frac{w}{2gR}.$ hyp. log. a. Which by substitution of numbers is = 305170·3 feet.

Hence it appears, that, after the burning of the rocket ceases, it will move to a distance of 305170·3 feet, or nearly 58 miles, before all its motion is destroyed, when it will remain at rest in the medium; there being no force to influence it in any manner or direction whatever, and having no power to create motion in itself. Space described.

As to the time that the rocket would be in moving through this space, it will be had as follows. The same substitution as above being retained; the general fluxional expression for the time (t') namely $\frac{-\dot{v}}{2gf}$ will be found $= \frac{-\dot{v}}{2gRv^2}$ = Time of moving through it.

$= \frac{-1}{2gR} \cdot \frac{\dot{v}}{v^2}$ (substituting $\frac{Rv^2}{w}$ for f as before) the fluent of which is $t = \frac{1}{2gRv}$. Now when $t = 0$, $v = a$, therefore the correct fluent of the time is $t = \frac{1}{2gRv} - \frac{1}{2gRa}$ which, on v becoming nothing, will be infinite. So that it appears, that the rocket will not describe the above space but in an infinite time.

Suppose $v = 1$ foot; then $t = \frac{a-1}{2gRa} = 133{\cdot}344$ seconds or 2 min. 13 seconds. That is, the rocket will only have been in motion 2 min. 13 sec. after it has acquired the greatest velocity from its burning before the celerity of its motion will be reduced to 1 foot per second; and yet, notwithstanding this great annihilation of velocity in so short a time, the remaining small part will not in any finite time be destroyed, though we know the limit at which the rocket would attain a state of quiescence.

A projected body cannot lose all its motion in any finite time.

And from the result here determined we conclude, that into whatever medium a body is projected with any given velocity, great or small, it will in no finite time lose all its motion. So that, if the planetary bodies were moving in a resisting medium, and gravity should suddenly be destroyed; the bodies would all pursue rectilinear paths (that would be tangents to their orbits) to certain finite distances, which would not be wholly described by them but in infinite times.

Lemma I.

Resistance to a cylinder moving perpendicularly through a fluid.

To determine the Resistance a Cylinder meets with in a Fluid when moving in a Direction perpendicular to its Axis.

It is universally allowed, and indeed is evident, that the resistance to a body moving through an infinite fluid at rest (such as is here supposed) is the same in effect as the force of the fluid in motion with equal velocity on the body at rest: therefore, as it will be somewhat more convenient to consider the fluid in motion, and the body quiescent, we shall pursue the solution of the problem upon this hypothesis.

Let

Let A B C D (Pl. VII, fig. 1) be the cylinder, and E T F any section parallel to the base. Let a particle strike this section at T in the direction P T, perpendicular, by supposition, to BD; and draw T O to the centre O: draw also the tangent T Q to the circle E T F or cylinder at T, upon which let fall the perpendicular P Q, and let fall the perpendicular Q R upon T P. Resistance to a cylinder moving perpendicularly through a fluid.

Then, considering P T to represent the full force of a particle of the fluid, P R will denote that part only, which has effect in moving the cylinder in the direction P R. For, on account of the obliquity of the surface of the solid, the stroke of the particle will also be oblique; and therefore, resolving T P into the two forces P Q and T Q, the force T Q only will be effective, which, in the direction P R, will be as P R, or the sine of the angle P Q R, or P T Q; as is evident by considering P Q resolved into the two forces Q R, R P; whereof the former, being parallel to the cylinder, has no effect in moving it in a perpendicular direction thereto.

Now by the nature of fluids, the force with which a particle strikes a body perpendicularly is equal to the weight of a line of such particles, the height of which is equal to that which is due to the velocity of its motion, or through which a body must fall to acquire that velocity; therefore, calling n the density of the particles or fluid; $\frac{nv^2}{4g}$ (where v denotes the velocity, and $g = 16$ feet) will be the absolute force of a particle moving with the velocity v. And this is represented above by the line P T; therefore, since rad. (1) : T P :: sin. $\angle$ P T Q (s) : P Q, the force denoted by P Q will be $\frac{nv^2s}{4g}$, and that by P R $\frac{nv^2s^2}{4g}$.

Put S L $= x$, L T $= y$, and S T $= z$; also let T n ($= \dot{z}$) express the fluxion of the course S T; then, because of the inclination of this line to the direction of the fluid, the number of particles striking it will be diminished in the ratio of T n to no, or of radius to the sine of the angle o T n; consequently the fluxion of the force of the fluid against S T, which would otherwise have been $\frac{nv^2s^2\dot{z}}{4g}$, will be $\frac{nv^2s^3\dot{z}}{4g}$.

Now

Resistance to a cylinder moving perpendicularly through a fluid.

Now $\dot{z} = (\dot{x}^2 + \dot{y}^2)^{\frac{1}{2}}$; and $y = (2rx - x^2)^{\frac{1}{2}}$ by the property of the circle; consequently $\dot{y} = \frac{r\dot{x} - x\dot{x}}{(2rx - x^2)^{\frac{1}{2}}}$ and $\dot{z} = (\dot{x}^2 + \dot{y}^2)^{\frac{1}{2}} = \frac{r\dot{x}}{(2rx - x^2)^{\frac{1}{2}}}$ (r being the rad. of the cir. ESF). Also, by reason ot similar triangles, $\frac{\mathrm{QP}}{\mathrm{TP}} = \frac{\mathrm{LT}}{\mathrm{OT}} = \frac{y}{r}$: whence s, being $= \frac{\mathrm{QP}}{\mathrm{TP}}$, will also be equal to $\frac{y}{r}$. Therefore by substitution $\frac{nv^2 s^3 \dot{z}}{4g} = \frac{nv^2}{4g} \cdot \frac{y^3}{r^3} \cdot \frac{r\dot{x}}{(2rx - x^2)^{\frac{1}{2}}} = \frac{nv^2}{4g} \cdot \frac{(2rx - x^2)^{\frac{3}{2}}}{r^3} \cdot \frac{r\dot{x}}{(2rx - x^2)^{\frac{1}{2}}} = \frac{nv^2}{4gr} \cdot (2rx\dot{x} - x^2\dot{x})$; of which the fluent is $\frac{nv^2}{4gr^2} \cdot \left(\frac{3rx^2 - x^3}{3}\right)$ wanting no correction; so that when $x = 2r$ the fluent will be $\frac{nv^2 r}{3g}$; which is the effective force of the fluid on the semicircumference of a section of the cylinder parallel to the base. Consequently $\frac{nv^2 r}{3g}$ into the height of the cylinder (h) $= \frac{nv^2 rh}{3g}$ will be the resistance, that the whole cylinder suffers when it moves in a direction perpendicular to its axis with the velocity v.

Cor. Because it is found, that a sphere, the radius of which is r, moving in a fluid of the density n, with the velocity v, is $\frac{pnv^2 r^2}{8g}$; we shall have the resistance of the sphere to the resistance of its circumscribing cylinder as $\frac{pnv^2 r^2}{8g}$ to $\frac{2nv^2 r^2}{3g}$, or as 1 to $\frac{16}{3p}$ (where $p = 3{\cdot}1416$); the latter therefore being resisted more than the former by about ·69829 of the former. Whence, the resistance to a sphere being given, the resistance to its circumscribing cylinder will be had by multiplying the former by 1·69829.

LEMMA

Lemma II.

To determine the same as in the last, when the Cylinder moves in any Direction oblique to its Axis.

Resistance to a cylinder moving obliquely.

Let T P (Pl. VII, fig. 2) be the direction of the cylinder moving in the fluid, or P T that of the fluid against the cylinder. Let a particle strike the solid at T, at which point draw the tangent T n to the section E F T, which is parallel to the base C D: draw L T Q perp. to the diameter V O S, which is at right angles to the axis X Y, and P Q and Q R perp. to T Q and T P respectively. Then, denoting the force of a particle of the fluid when in motion by P T, and supposing this to be resolved into the two forces P Q, Q T, the latter only, Q T, which varies as the sine of the angle T P Q, will have effect in moving the cylinder; which, in the direction P T, will be as R T, or the sine of the angle T Q R, or S P Q. Now the effective force of a particle in the direction Q T has been shown in the preceding lemma to be equal to $\frac{n v^2 s^2}{4 g}$ when the whole force of a particle is represented by Q T: but in the case before us, putting f for the sine of the angle Q P T, or of the angle of incidence of the impingeing fluid against the solid, the efficacy of Q T will consequently be $= \frac{n v^2 s^2 f}{4 g}$ (where s = sin. of the angle Q T n) and therefore the effect of a particle to move the cylinder in the direction P T will be $\frac{n v^2 s^2 f^2}{4 g}$.

Put c = sin. of the angle P T Q, the cosin. being f

r = rad. of the base of the cylinder

x = O L

y = T L

Then, by reason of the similitude of the triangles O L T, T n K, we obviously obtain $s = \frac{y}{r} = \frac{(r^2 - x^2)^{\frac{1}{2}}}{r}$, and the

Resistance to a cylinder moving obliquely.

the cosine of the angle K T n $= \frac{x}{r}$; (radius being unity in all cases). Now let $z =$ F T and $\dot{z}$ the fluxion of the same, then the fluxion of the force of the fluid on F T will be $= \frac{n v^2 s^2 f^2 \dot{z}}{4g}$ multiplied by the sine of the angle P T n, whereof the angle P T n being composed of the two angles P T Q, Q T n, the natural sines and cosines of which are represented above; its sine by trig. will be expressed by $\frac{cx}{r} + \frac{f(r^2 - x^2)^{\frac{1}{2}}}{r} = \frac{cx + f(r^2 - x^2)^{\frac{1}{2}}}{r}$; also $\dot{z}$ $= \frac{r\dot{x}}{(r^2 - x^2)^{\frac{1}{2}}}$. Therefore

$$\frac{n v^2 s^2 f^2 \dot{z}}{4g} \cdot \text{sine angle P T } n =$$

$$\frac{n v f^2}{4g} \cdot \frac{r^2 - x^2}{r^2} \cdot \frac{r\dot{x}}{(r^2-x^2)^{\frac{1}{2}}} \cdot \frac{cx + f(r^2 - x^2)^{\frac{1}{2}}}{r}$$

$$= \frac{n v^2 f^2}{4gr^2} \cdot \frac{\left\{cx\,(r^2 - x^2) + f\,(r^2 - x^2)^{\frac{3}{2}}\right\}\dot{x}}{(r^2 - x^2)^{\frac{1}{2}}}$$

$$= \frac{n v^2 f^2}{4gr^2} \cdot \left\{cx\dot{x}\,(r^2 - x^2)^{\frac{1}{2}} + fr^2\dot{x} - fx^2\dot{x}\right\};$$

the fluent of which is

$$\frac{n v^2 f^2}{4gr^2} \cdot \left\{ - \frac{1}{3} c\,(r^2 - x^2)^{\frac{3}{2}} + fr^2 x - \frac{1}{3} f x^3 \right\}$$

which corrected will, in the ultimate case, where $x = r$, be

$$\frac{n v^2 f^2}{4gr^2} \cdot \left(\frac{1}{3} c r^3 + \frac{2}{3} f r^3 \right) = \frac{n v^2 f^2 r}{12g} \cdot \left(c + 2f \right),$$

which is therefore the effective force of the fluid on the quadrantal arch FTS. Hence $\frac{n v^2 f^2 r}{6g} \cdot (c + 2f)$ will be the force on the semicircum. V F S; and $\frac{n v^2 f^2 r h}{6g} \cdot (c + 2f)$ the force on the whole semicylindric surface $=$ D v r B s; or

or the resistance to the cylinder when moving in the fluid at rest, so far as relates to that surface. Resistance a cylinder moving obliquely.

To determine what farther resistance is opposed to the cylinder by the fluid acting against the top $A\,s\,B\,r$. Let us suppose AVBT (fig. 3) to be the head of the cylinder, and a particle striking it at T; also let AB be a diameter of the circle perp. to the axis, and draw TQ parallel to AB, and PQ and QR perp. to TQ and TP respectively. Then PT being considered the representative of the full force of a particle, and to be resolved into the two forces PQ, TQ; the force TQ, being parallel to the plane ABV, has no effect in causing it to move; but only the force denoted by PQ, which is as the sine (c) of the angle PTQ. Therefore the effective force of a particle in this case will be $\frac{nv^2c^2}{4g}$; and that of the fluid on the whole circular plane $\frac{nv^2c^3pr^2}{4g}$ (p being $= 3{\cdot}1416$). Hence the whole resistance to the cylinder is

$$\frac{nv^2f^2rh}{6g}\cdot(c+2f)+\frac{nv^2c^3pr^2}{4g}$$

Cor. 1. When the angle TPQ (fig. 2) is 90°, or the solid moves in a direction perp. to its axis; then f becoming 1 and c nothing, the resistance to the cylinder will be $\frac{nv^2rh}{3g}$ as determined in the first lemma.

Cor. 2. The resistance to the cylinder moving in the direction TP estimated in the direction QT is $\frac{nv^2frh}{6g}$ $(c+2f)$, being that arising only from the action of the fluid upon the semisurface of the solid; that on the head or top of the cylinder having no effect to move it in this direction, but in the direction of its axis.

For an example to this proposition in numbers, when the medium is supposed to be that of our atmosphere. Let the angle TPQ (the sine of which is f) $= 60°$; and consequently the angle PTQ (the sine of which is c) $= 30°$.

Then

Then we have $\int = \cdot 866$

$c = \cdot 5$

Let $r = 6$ in. $= \frac{1}{2}$ foot

$v = 1$ ft.

$h = 3$ ft.

$g = 16$ ft, (omitting the $\frac{1}{12}$th)

$n = 1\frac{1}{3}$

$$\text{Hence } \frac{n v^2 \int^3 r h}{6 g} \cdot (c + 2\int) + \frac{n v^2 c^3 p r^2}{4 g} =$$

$\cdot 03196687 + \cdot 00187486 = \cdot 03384173$ ounces for the resistance to a cylinder of the above dimensions, when moving with the velocity of 1 foot per second. And therefore, as the resistance to the same cylinder varies as the square of the velocity, the resistance corresponding to any other velocity will be had by multiplying the above by the velocity (in feet) squared.

II.

On the Defective Algorithm of Imaginary Quantities. In a Letter from a Correspondent.

To Mr. NICHOLSON.

SIR,

A quadratic equation apparently with three roots.

IN a mathematical investigation, in which I was lately engaged, I fell upon a very singular anomaly in the theory of equations, which is nothing less than a *quadratic equation* having (at least to all appearance) *three roots*, all different from each other; whereas, according to received principles, it can have only two. As this is a very strange deviation from what has been hitherto considered as a well established theory, I am induced to request the publication of it in your Journal, in hopes that some of your mathematical correspondents may undertake to explain the difficulty, and rescue the theory of equations, and the present algorithm of imaginary quantities, from the danger to which such anomalies must necessarily expose them; particularly as there are

[redacted]

are some among us, who wish to cramp the power of analysis, by rejecting in that science every species of quantity coming under an imaginary form. I think I can perceive where the mystery lies; but still I should be glad to see the opinion of more able analysts on this apparent incongruity; if however no such should appear, I will, through the medium of your Journal, publish my ideas on the subject.

The equation to which I have alluded is this:

$$x^2 + x = 2$$

and the *three roots* of it are the following,

$$\begin{aligned} &\text{1st root } x = 1 \\ &\text{2d root } x = -2 \\ &\text{3d root } x = \sqrt[3]{\tfrac{1}{2} + \sqrt{-\tfrac{3}{4}}} + \sqrt[3]{\tfrac{1}{2} - \sqrt{-\tfrac{3}{4}}} \end{aligned}$$

The two first of which evidently answer the conditions of the equation, and with the third I proceed as follows.

$$x^2 = \left(\sqrt[3]{\tfrac{1}{2} + \sqrt{-\tfrac{3}{4}}} + \sqrt[3]{\tfrac{1}{2} - \sqrt{-\tfrac{3}{4}}}\right)^2, \text{ or}$$

$$x^2 = \sqrt[3]{(\tfrac{1}{2} + \sqrt{-\tfrac{3}{4}})^2} + 2\sqrt[3]{(\tfrac{1}{2} + \sqrt{-\tfrac{3}{4}}) \times (\tfrac{1}{2} - \sqrt{-\tfrac{3}{4}})}$$

$$+ \sqrt[3]{(\tfrac{1}{2} - \sqrt{-\tfrac{3}{4}})^2}$$

And now in order that I may be certain of my results, I multiply these quantities under the radicals at full length, as follows; viz.

$$\left.\begin{array}{l} \tfrac{1}{2} + \sqrt{-\tfrac{3}{4}} \\ \tfrac{1}{2} + \sqrt{-\tfrac{3}{4}} \end{array}\right\} \text{to find the square of } \tfrac{1}{2} + \sqrt{-\tfrac{3}{4}}$$

$$\begin{array}{l} \tfrac{1}{4} + \tfrac{1}{2}\sqrt{-\tfrac{3}{4}} \\ \qquad \tfrac{1}{2}\sqrt{-\tfrac{3}{4}} - \tfrac{3}{4} \\ \hline \tfrac{1}{4} + \sqrt{-\tfrac{3}{4}} - \tfrac{3}{4} = -\tfrac{1}{2} + \sqrt{-\tfrac{3}{4}} = -(\tfrac{1}{2} - \sqrt{-\tfrac{3}{4}}) \end{array}$$

$$\left.\begin{array}{l} \tfrac{1}{2} + \sqrt{-\tfrac{3}{4}} \\ \tfrac{1}{2} - \sqrt{-\tfrac{3}{4}} \end{array}\right\} \text{for the product } (\tfrac{1}{2} + \sqrt{-\tfrac{3}{4}}) \times (\tfrac{1}{2} - \sqrt{-\tfrac{3}{4}})$$

$$\begin{array}{l} \tfrac{1}{4} + \tfrac{1}{2}\sqrt{-\tfrac{3}{4}} \\ \quad -\tfrac{1}{2}\sqrt{-\tfrac{3}{4}} - -\tfrac{3}{4} \\ \hline \tfrac{1}{4} \qquad * \qquad + \tfrac{3}{4} = 1 \end{array}$$

$$\left.\begin{array}{l}\frac{1}{2} - \sqrt{-\frac{3}{4}} \\ \frac{1}{2} - \sqrt{-\frac{3}{4}}\end{array}\right\} \text{to find the square of } \frac{1}{2} - \sqrt{-\frac{3}{4}}$$

$$\begin{array}{l}\frac{1}{4} - \frac{1}{2}\sqrt{-\frac{3}{4}} \\ \quad - \frac{1}{2}\sqrt{-\frac{3}{4}} - \frac{3}{4} \\ \hline \frac{1}{4} - \sqrt{-\frac{3}{4}} - \frac{3}{4} = -\frac{1}{2} - \sqrt{-\frac{3}{4}} = -\left(\frac{1}{2} + \sqrt{-\frac{3}{4}}\right)\end{array}$$

Therefore

$$x^2 = \sqrt[3]{-\left(\frac{1}{2} - \sqrt{-\frac{3}{4}}\right)} + 2 + \sqrt[3]{-\left(\frac{1}{2} + \sqrt{-\frac{3}{4}}\right)} \text{ or}$$

$$x^2 = 2 - \sqrt[3]{\frac{1}{2} - \sqrt{-\frac{3}{4}}} - \sqrt[3]{\frac{1}{2} + \sqrt{-\frac{3}{4}}} \text{ or}$$

$$x^2 = 2 - \left(\sqrt[3]{\frac{1}{2} + \sqrt{-\frac{3}{4}}} + \sqrt[3]{\frac{1}{2} - \sqrt{-\frac{3}{4}}}\right) \text{ but}$$

$$x = \left(\sqrt[3]{\frac{1}{2} + \sqrt{-\frac{3}{4}}} + \sqrt[3]{\frac{1}{2} - \sqrt{-\frac{3}{4}}}\right)$$

and therefore by addition, we have

$$x^2 + x = 2$$

Now if this be a legitimate result, I see no reason why this value of x should not be considered as a root of the proposed equation as well as the other two; and if it be admitted as such, then I can find any number of other roots at pleasure; which will totally destroy the established theory of equations; but if, on the contrary, this cannot be admitted as a root, then it necessarily follows, that the present algorithm of imaginary quantities is defective, or otherwise that I have deviated from that algorithm in the preceding operation. In order to discover the errour wherever it may lie, and that the connection of it may be made public, I am induced to request the publication of this paper in your Journal; which, if you should think proper to comply with, will much oblige

Yours &c.

MATHEMATICUS.

III.

III.

On the Nature of Heat. By MARSHALL HALL, *Esq.* *In a Letter from the Author.*

(Concluded from p. 222.)

IN appreciating the merit of any hypothesis, we ought certainly to consider, what assumptions are inseparable from the subject itself; and what suppositions are necessary, to constitute the particular hypothesis proposed.

Some assumptions may be inseparable from a subject, others connected with a particular view of it.

To apply this to our subject; it appears to me, that, whatever may be our notion concerning the ultimate nature of caloric, one postulate must necessarily be made; the existence of a channel for this agent between the Sun and the Earth must unavoidably be assumed.

Suppositions connected with the materiality, and immateriality of heat.

If we embrace the opinion of the materiality of caloric, we suppose, that this matter emanates constantly from the sun's surface; and penetrates space. On the other hand, in adopting the opposite opinion, we necessarily suppose the existence of a fluid, naturally pervading the universe in a state of quiescence; but ready to be impressed by external causes. This is indeed the great difficulty; and a difficulty, which no one will pretend to obviate. It may diminish the objection, which is thus afforded to the hypothesis, to observe, that on either side of the question the difficulty is nearly the same; or, if there be any difference, it is in favour of the hypothesis of vibration. For what is the great difference, between the assumption of a material agent, which, being impelled, penetrates space with rapid motion; and that of a quiescent fluid pervading space, and subject to certain impressions?

Most assumptions in the material hypothesis.

But, if we consider this circumstance farther, we shall observe, that, in the material theory, the assumption of one fluid only does not suffice. According to this opinion, the sun-beam must consist of at least three; or, if we consider the compound nature of light, of no less than of nine distinct fluids. Many persons however will be willing to grant all these to modern theorists, who would refuse to Huygens and

and Euler their ethereal medium; although both suppositions are equally hypothetical.

Neglect of induction

It must be acknowledged, that in investigating the nature of caloric, we subject ourselves to the imputation of false philosophy. We neglect the method of induction, and seek, as the ancients did, occult causes. So much are we involved in the trammels of theory, that we are scarcely able, in some sciences, to express a single fact, without implying the existence of something perfectly hypothetical; this is very much the case with our present subject, heat, and with the science of electricity. We are educated in the belief of such hypotheses, and do not doubt of their truth, until a considerable progress has been made in the study of them. It is not one of the least of the uses of investigations like the present, to teach us how very little all hypotheses ought to be relied on, and how very much and how constantly they ought to be distrusted.

and recourse to hypothesis too common.

The Nature of the Vibration of Heat.

Nature of the vibration of heat.

Heat may possibly depend, not on the presence of any material fluid in the interstices of bodies, but on a state of intimate vibration of their particles. The temperature or degree of heat may be greater or less, according as these vibrations may be more or less frequent in any given time; or, as it may be expressed, according to the intensity of the vibration. By this term I wish only to express the relative state of the vibration; that vibration I suppose to be the most intense, which occasions the highest temperature.

Objection to the hypothesis.

An objection which has always been urged to the hypothesis of vibration is, that the propagation of heat does not obey the established laws of motion. "Were they the same, its propagation ought to be momentary through elastic bodies, and should be more or less rapid through others, according to their elasticity."

Answer to the first part of it.

To the first part of this objection, it may be answered, that the propagation of heat through elastic bodies is indeed momentary; for this is the radiation of caloric. Previously to considering the second part of the objection, it will be necessary to consider somewhat of the *nature* of the vibrations,

tions, which we have supposed to constitute heat; and to show in what respect they differ from other vibrations.

Difference between the vibrations of heat and sound.

The important and manifest difference between these vibrations, of sound, for example, and of heat, is, that in the former the *mass*, in the latter, the *particles* only of that mass, vibrate; and this distinction is sufficient to explain the necessary and consequent difference in the laws observed by these vibrations. The facility with which the mass of any body vibrates will be proportionate to the elasticity of the body; but it is plain, that the vibration of the particles of the mass will obey laws as different as the vibrations themselves are different: accordingly, who, after this consideration, would expect that the elasticity of any body should regulate the vibration of its particles only? It is argued indeed, that the vibration of the mass of any body must ultimately be referred to the condition of its particles; this I readily admit: yet it proves nothing; it does not prove that the converse of this is true; namely, that the vibration of the particles must be determined by the condition of the mass.

Other reflections.

Perhaps it was the want of considering the necessary difference between the vibrations of heat and of sound, that has led to some other objections to this theory. It has been said, that no body could communicate heat to another, (if heat were vibration), unless the second made a sort of concord with the first. Another objection is still more futile; the vibrations, if such constituted heat, would, it is said, "gradually relax and die away."

Sources of Caloric.

Sources of heat.

For the same reason, that this part of the subject was treated with brevity in the former part of this discussion, I may be equally concise in this place. The intimate connection between motion, friction, percussion, &c. and heat, has lately been so much attended to, and so satisfactorily explained by the theory of vibration, that nothing scarcely remains to be added on this point.

The light and heat produced by the transition of the electric fluid from one body to another is extremely analogous to the sound produced by the motion of air through tubes.

If the transition be sudden, powerful light and heat are occasioned; if it be slow and equable, a continual spring of light and heat is formed. In the same manner, if the motion of air be rapid, a sound is produced as powerful as the heat and light, in the first instance; if the motion of the air be slow and equable, the sound produced is smooth and uninterrupted.

Motion of Caloric.

Motion of heat.

It has been said, that the best conductors of caloric receive and part with this power the most rapidly. This is precisely what our hypothesis would have led us to expect, *a priori*. It is to be remarked, that the action of hot and cold bodies upon each other is *reciprocal*. The heating and cooling of bodies is, according to our opinion, the same operation; both are reducible to the effecting a change in the state of vibration; and different substances are susceptible of this change in different degrees; those which are most so are the most easily heated, and the most readily cooled.

This explanation applies equally well to the absorption and radiation of heat and cold; which are perhaps greater difficulties in the opposite opinion, than even the circumstance with respect to conducting power.

Radiant heat.

It was formerly stated, that radiant heat is extremely different, according as it comes from the sun, or from a source of heat upon Earth; I wish however to state this difference somewhat more distinctly.

From the sun.

That the heat of the sun is transmissible through and refrangible by transparent media, is abundantly proved; the refrangibility of the heat accompanying the coloured part of the prismatic spectrum, and of the invisible rays of solar heat, is shown in the 13th and 17th experiments of Dr. Herschel *; and the familiar use of burning lenses demonstrates the refraction of the caloric of the undecomposed solar ray.

From a fire.

On the other hand, that culinary heat is not transmissible through any solid body is very decisively proved by the valuable experiments contained in Mr. Leslie's third chapter: see the Inquiry.

* Phil. Trans. for 1800: or Journal, 4to Series, Vol. IV, p. 364, 366.

The

The latter statement requires however some qualification and restriction; for I must now observe, that, although Mr. Leslie's experiments prove decidedly the difference between solar and culinary heat in this respect, yet he has I believe proceeded too far, in asserting, that the latter is not *at all* transmissible through transparent media. That the heat of a candle is in some degree refracted by glass is proved by the 13th experiment of Dr. Herschel; the heat of a common fire was transmitted and refracted in the 14th and 16th; the heat of redhot iron was refracted in the 15th; and invisible culinary heat was refracted in the 19th and 20th experiments*. The heat emanating from a candle, from a boiling mixture of sulphuric acid and water, and from boiling water, was transmitted through glass, in some experiments performed by my friend Mr. Maycock †.

The latter i some degree transmissible,

The whole of these experiments concur in establishing a remarkable difference between the transmission of radiant culinary and solar heat. Solar heat is scarcely if at all impeded, culinary heat almost entirely intercepted by transparent media!

but not equall to solar heat.

But this is not the only difference between solar and culinary heat; another distinction is observed in their reflection. "Cover each ball of a differential thermometer with a coat "of tinfoil, and rub that one below which the scale is affixed gently with sand paper; or it may be rubbed before "it is applied to the glass. Placing the instrument now in "the sun, the liquor will visibly rise, perhaps 5 or 10 degrees." "Set this differential thermometer now directly "opposite the fire, and about two or three feet distant from "it. In this situation a very remarkable depression will "quickly take place, equal perhaps to 30 or 40 degrees." "This beautiful experiment likewise indicates clearly the "distinction between the solar rays and culinary heat ‡."

Difference in the reflection of solar and culinary heat.

The explanation of this phenomenon, which follows its relation, will not, I conceive be readily acquiesced in; "the "light from the fire, has," it is said, "some tendency, to

Attempt to ac count for thi

* Phil. Trans. 1800; or Journal as above.

† Phil. Journal, Vol. XXVI, p. 75.

‡ Inquiry, p. 83, et seq.

"counteract

"counteract or diminish in a certain measure the peculiar "effect of the heat emitted from the same source."

Other differences.

Another difference still between the two kinds of heat was discovered by Mr. Leslie. A very considerable aberration takes place in the reflection of culinary heat, which is not I believe the case with the solar rays. Nor is the effect of colour, in absorbing the two kinds of heat, the same.—"Stained paper has very nearly the same action as white "paper, and it is only when covered by a pigment super-"induced, that the diversity of effect becomes conspicu-"ous*."

Why solar heat passes through transparent bodies wholly;

I shall now attempt to explain this remarkable difference between solar and culinary heat. Solar heat may consist of vibrations in that medium or fluid, which we suppose to fill space. This fluid is one of extreme tenuity, and pervades all bodies without exception; vibration therefore, which subsists in this fluid, does and ought to pass through such bodies as are transparent, with little or no interruption.

other heat only in part.

Radiation from other bodies, that is radiant culinary heat, is very different: the radiator is in a state of vibration; this vibration is communicated to all surrounding bodies, the most important of which is the atmosphere; the subtile fluid too must be taken into consideration; these, with other bodies, which are within the vicinity of the source of heat, take on vibration, and convey it to distant surfaces. In as much as the vibration subsists in the more subtile medium, it will, as it did in the case of solar heat, pervade transparent bodies; but the chief conductor of heat in this operation is the "ambient air;" this fluid does not pervade transparent or other bodies, its vibrations will therefore be intercepted by them.

Other differences.

It is easy to conceive, that, as the two kinds of radiant heat are so extremely different, the laws which are observed in their other motions shall be very different; a difference in their reflection and absorption is what might have been naurally expected.

Opaque bodies.

From some cause, the pervading fluid of certain bodies does not propagate its vibrations; these bodies are therefore

* Inquiry, p. 94.

opaque

opaque*. It is from this circumstance, that opaque bodies only are heated by the sun's rays; in intercepting them, they receive the heating power, the vibration of these rays. Such bodies act upon solar heat somewhat in the same manner as all bodies act upon culinary heat.

Air a quick conductor of heat.

From this view of the subject it appears, that air is a very quick conductor of heat; Berthollet has remarked this fact†: nevertheless air is employed in the arts as a bad conductor: this circumstance requires some explanation.

Conducting power of bodies.

Different bodies are susceptible, in different degrees, of undergoing a change in their vibration; and, having suffered a change in their vibration, they convey this change to distant parts with different degrees of celerity. Cæteris paribus, those bodies, which are most susceptible of change in vibration, induce the least change in other bodies; and, cæteris paribus, those bodies, which convey the changes they may have suffered with most celerity, produce the greatest change in other bodies. Thus the conductor, which occasions the greatest change in temperature, is that which unites the properties of celerity of conducting power and little susceptibility of change in vibration: and thus, although air conducts vibration with much celerity, yet, from its high susceptibility of change in vibration, its effect in augmenting or reducing the temperature of bodies is by no means great. It appears that the terms good and bad conductors are involved in some ambiguity.

Radiation of Cold.

Radiation of cold.

This phenomenon appears to me to be the most decisive in demonstrating the true nature of caloric; it deserves perhaps the appellation of experimentum crucis.

Effect of a concave mirror.

A concave mirror has the property of concentrating the rays of vibration proceeding from a source properly opposed to it. In a similar manner the vibrations of air constituting sound are converged in an eliptical chamber. The operation of mirrors does not however increase the *intensity* of the

* It is necessary to remark, that I have considered the theory of light of Huygens and Euler as the most probable; a few observations on this subject may probably at some future time be transmitted to the Philosophical Journal.

† Murray, Vol. I, p. 274.

vibration

vibration of rays, it merely causes them to converge, collects and unites their effect. The intensity of vibration in the focus of the mirror is not greater than that of each of the rays before they converged; but as the force of all the rays is concentrated in the focus, the heating effect will be greater there; that is, a body in the focus will be heated much sooner than by the operation of a single ray, but will never attain to an intensity of vibration greater than that of a single ray. In like manner, although the force of rays of sound be accumulated in the focus of an elliptical chamber, yet the note, or pitch of the sound, i. e. its intensity of vibration, remains the same.

Now vibrations of a certain intensity occasion the sensation and phenomena of cold; the accumulation of rays of vibration of this intensity by means of a concave mirror, as before, does not alter their intensity, but merely converges and collects their force, and thus increases the effect of producing cold; and this it does, to the very same extent, provided all circumstances be equal, as the effect of producing heat was increased by converging the rays of heat. I have endeavoured to ascertain this by experiment. The temperature of the atmosphere was 60°. Two mirrors were properly opposed to each other; in the focus of one was placed a thermometer, in that of the other a cubical canister, one side of which, (namely, that opposed to the mirror), was blackened. The canister was now filled with water at 90°. The effect on the thermometer in time and extent was marked: the canister was then removed, and its place supplied by a similar one containing a saline solution at 30°. The effect on the thermometer was opposite, but equal in time and in degree, to that of the former experiment.

Experiment.

This fact is of an importance not to be easily appreciated; it appears to me to identify heat with vibration.

Effects of Heat.

Effects of heat.

In the former part of my paper I have related some facts, which are not only inexplicable on the theory of repulsive caloric, but which appear to afford some degree of contradiction to it; it will therefore appear, that an explanation of

of these facts must necessarily proceed on some other principle.

It is certain, that two energetic, but opposite powers, are constantly active, in all the operations of chemistry; these are attraction and repulsion. The nature however of these powers is still very uncertain: the effects of heat are all referrible to changes in their condition; but from our ignorance of their nature, it must be extremely difficult, to ascertain with precision the cause and nature of the changes they undergo.

Attraction and repulsion.

The phenomena which I have mentioned as contradictory to the theory of repulsive caloric have been ascribed to the agency of a certain polarity in the particles of the body; by means of this polarity the particles are opposed to each other in a particular manner; and the state of attraction and repulsion is influenced or regulated by this state of apposition. The change in temperature is the cause of the change in the apposition of the particles; and this change of apposition of the particles proves the cause of the change in the state of attraction and repulsion, and consequently of the bulk of the body.

Accounted for by polarity of particles.

This explanation is probably correct; and if it apply in one instance of changes produced by temperature, why not in all? The greatest density of water seems to be about the temperature of 38°. If its temperature suffer any change from this point, expansion occurs; and for any given number of degrees above or below this temperature, the expansion is the same, if the water retain the fluid force. Here therefore, the effects are precisely similar, but, according to the theory, they are ascribed to *causes* that are different; which in itself appears to me contrary to the true laws of philosophising. This opinion therefore, and the objections which I have mentioned to the usual explanation, have induced me to refer the changes of bulk from temperature, in every case, to the same cause; whatever the cause may be.

Change of bulk from temperature.

There are many circumstances, which tend to corroborate the idea of polarity. The appearances of crystallization appear to depend on its agency. The state of fluidity of bodies must also be referred to the " particular situation" of their particles,

Circumstances in favour of the hypothesis of polarity.

particles. And I can conceive, that the agency of attraction, whether of aggregation or of composition, may in every case be influenced or regulated by the particular state of apposition of particles.

It will readily be acknowledged, that much difficulty and uncertainty still exist in this question; but I conceive, that the difficulties are incomparably greater in relation to the theory of repulsive caloric, than in the view of the subject, which has been given. If we could explain the cause of the expansion of water, cooled from 40° to 10°, we should probably find little difficulty in understanding the similar and precisely equal expansion, when the same water is raised in its temperature from 40° to 70°.

Expansion of water.

Capacity for Caloric.

Capacity for heat.

It would be extraordinary indeed, if all bodies were equally susceptible of vibration; no property of matter is equally possessed by all the innumerable substances, which nature presents to our attention; gravity, hardness, elasticity, &c. are possessed in an equal degree by no two bodies with which we are acquainted: such is the diversity in Nature's works! Nor are all bodies equally susceptible of change in the state of their vibration. This proposition is sufficient to account for the variety in the capacity of different bodies, and of the same body under different forms, for heat. Mercury is more susceptible of vibration than water; solids than fluids; fluids than gasses; the quantities, for comparison, being ascertained by weight*.

Mercury and water.

Let mercury at 40° be mixed with an equal weight of water at 80°; mercury is more susceptible of change in the state of its vibration than water, and will consequently suffer more change; its intensity of vibration will pass more nearly to that of the water, than the intensity of vibration in the latter will to that of the mercury: the resulting temperature will therefore be above the mean; i. e. more nearly that of the water than the mean. If the experiment be reversed, the effect will also be reversed.

Change in capacity.

If during the time of the change in the susceptibility of any body for vibration (this change being to diminish its

* In speaking formerly of the high susceptibility of air for change in vibration its quantity was considered by bulk, not by weight.

susceptibility

susceptibility) heat be communicated, its temperature may remain unaltered; a greater power, or longer application of vibration, being now necessary to occasion a temperature, which, before the susceptibility for vibration was diminished, was produced by a power much smaller, or an application much shorter. Hence steam is no higher in temperature than boiling water. If, during this change of susceptibility for vibration, no farther application of heat be made, it follows, that the temperature must fall: hence arise the effects of freezing mixtures.

Steam.

Freezing mixtures.

It scarcely need be added, that the converse of all this will take place, if the susceptibility for vibration be increased, and no abstraction of heat be made. The temperature then must rise; for the body contains within itself what may be termed the power of vibration; a given quantity of which produces a greater intensity of vibration in any body, according to the susceptibility of that body for vibration.

Converse of this.

Such is an imperfect sketch of the hypothesis of vibration, which I proposed to give. Many circumstances, which would have elucidated, and perhaps have confirmed the opinions, have been necessarily omitted; and here the greatest candour of your readers will be constantly required.

It may be useful in concluding, to present a summary of the circumstances which have been considered; and thus to institute a comparison between the two hypotheses.

Summary.

1st, The first principles of each opinion are equally hypothetical.

The two hypotheses compared.

2dly, The production of heat by friction is explained by the hypothesis we propose; but not, satisfactorily at least, by the other.

3dly, Certain facts have been related, under the head of the effects of heat, which appear to afford some degree of contradiction to the hypothesis of material caloric; and although they may not be easily explained on the opposite principle, yet they do not by any means appear contradictory to it.

The advantages of our theory appear most conspicuous in the following particulars; for

4thly, The properties of good conductors, and of good radiators of caloric, are explained by it alone.

5thly,

5thly, The same observation applies to the difference of solar and culinary heat;

6thly, And in particular to the radiation of cold.

7thly, The opinion of capacity for caloric is hypothetical; that of the difference in susceptibility for vibration is in conformity to the usual order of nature, in dispensing the other properties of matter.

I remain, Sir,

Your very obedient,

Edinburgh, June 8th, 1814. MARSHALL HALL.

IV.

On a Combination of Oximuriatic Gas and Oxigen Gas. By HUMPHRY DAVY, *Esq. LL. D. Sec. R. S. Prof. Chem. R. I.**

Compound of oxigen and oximuriatic gas.

I SHALL beg permission to lay before the Society the account of some experiments on a compound of oximuriatic gas and oxigen gas, which, I trust, will be found to illustrate an interesting branch of chemical inquiry, and which offer some extraordinary and novel results.

Oximuriatic gas differs when differently prepared.

I was led to make these experiments in consequence of the difference between the properties of oximuriatic gas prepared in different modes; it would occupy a great length of time, to state the whole progress of this investigation. It will, I conceive, be more interesting, that I should immediately refer to the facts; most of which have been witnessed by Members of this Body, belonging to the Committee of Chemistry of the Royal Institution.

Its properties when procured by means of manganese.

The oximuriatic gas prepared from manganese, either by mixing it with a muriate and acting upon it by sulphuric acid, or by mixing it with muriatic acid, is, when the oxide of manganese is pure, and whether collected over water or mercury, uniform in its properties; its colour is a pale yellowish green; water takes up about twice its volume, and scarcely gains any colour; the metals burn in it readily; it

* Phil. Trans. for 1811, p. 155.

combines

combines with hidrogen without any deposition of moisture: it does not act on nitrous gas, or muriatic acid, or carbonic oxide, or sulphureous gasses, when they have been carefully dried. It is the substance which I employed in all the experiments on the combinations of oximuriatic gas described in my last two papers.

The gas produced by the action of muriatic acid on the salts which have been called hyperoximuriates, on the contrary, differs very much in its properties, according as the manner in which it is prepared and collected is different. Varies when procured from hyperoximuriates.

When much acid is employed to a small quantity of salt, and the gas is collected over water, the water becomes tinged of a lemon colour; but the gas collected is the same as that procured from manganese.

When the gas is collected over mercury, and is procured from a weak acid, and from a great excess of salt, by a low heat, its colour is a dense tint of brilliant yellow green, and it possesses properties entirely different from the gas collected over water.

It sometimes explodes during the time of its transfer from one vessel to another, producing heat and light, with an expansion of volume; and it may be always made to explode by a very gentle heat, often by that of the hand*.

It is a compound of oximuriatic gas and oxigen, mixed with some oximuriatic gas. This is proved by the results of its spontaneous explosion. It gives off, in this process, from $\frac{1}{4}$ to $\frac{1}{6}$ its volume of oxigen, loses its vivid colour, and becomes common oximuriatic gas. A compound.

I attempted to obtain the explosive gas in a pure form, by applying heat to a solution of it in water; but in this case, there was a partial decomposition; and some oxigen Attempts to obtain it pure.

* My brother, Mr. J. Davy, from whom I receive constant and able assistance in all my chemical inquiries, had several times observed explosions, in transferring the gas from hyperoximuriate of potash, over mercury, and he was inclined to attribute the phænomenon to the combustion of a thin film of mercury, in contact with a globule of gas. I several times endeavoured to produce the effect, but without success, till an acid was employed for the preparation of the gas, so diluted as not to afford it without the assistance of heat. The change of colour and expansion of volume, when the effect took place, immediately convinced me, that it was owing to a decomposition of the gas.

was

was disengaged, and some oximuriatic gas formed. Finding that, in the cases when it was most pure, it scarcely acted upon mercury, I attempted to separate the oximuriatic gas with which it is mixed, by agitation in a tube with this metal; corosive sublimate formed, and an elastic fluid was obtained, which was almost entirely absorbed by $\frac{1}{4}$ of its volume of water.

Dangerous.

This gas in its pure form is so easily decomposable, that it is dangerous to operate upon considerable quantities.

In one set of experiments upon it, a jar of strong glass, containing 40 cubical inches, exploded in my hands with a loud report, producing light; the vessel was broken, and fragments of it were thrown to a considerable distance.

Analysis of it.

I analysed a portion of this gas, by causing it to explode over mercury in a curved glass tube, by the heat of a spirit lamp.

The oximuriatic gas formed, was absorbed by water; the oxigen was found to be pure, by the test of nitrous gas.

50 parts of the detonating gas, by decomposition, expanded so as to become 60 parts, The oxigen, remaining after the absorption of the oximuriatic gas, was about 20 parts. Several other experiments were made, with similar results. So that it may be inferred, that it consists of 2 in volume of oximuriatic gas, and 1 in volume of oxigen; and the oxigen in the gas is condensed to half its volume. Circumstances conformable to the laws of combination of gaseous fluids, so ably illustrated by Mr. Gay-Lussac, and to the theory of definite proportions.

I have stated on a former occasion, that approximations to the numbers representing the proportions in which oxigen and oximuriatic gas combine are found in 7·5 and 32·9. And this compound gas contains nearly these quantities*.

The

Spec grav. of

* In page 245 of the Phil. Trans. for 1810, (Journal, vol. XXVII, p. 333,) I have mentioned, that the specific gravity of oximuriatic gas is between 74 and 75 grains per 100 cubical inches. The gas, that I weighed, was collected over water, and procured from hyperoximuriate of potash, and at that time I conceived, that this elastic fluid did not differ from the oximuriatic gas from mangenese, except in being purer. It probably contained some of the new gas; for I find, that the specific gravity

The smell of the pure explosive gas somewhat resembles that of burnt sugar, mixed with the peculiar smell of oximuriatic gas. Water appeared to take up eight or ten times its volume; but the experiment was made over mercury, which might occasion an errour, though it did not seem to act on the fluid. The water became of a tint approaching to orange. Smell of the gas. Solubility.

When the explosive gas was detonated with hidrogen equal to twice its volume, there was a great absorption, to more than $\frac{1}{3}$, and solution of muriatic acid was formed; when the explosive gas was in excess, oxigen was always expelled, a fact demonstrating the stronger attraction of hidrogen for oximuriatic gas than for oxigen. Attraction of oximuriatic gas for hidrogen.

I have said that mercury has no action upon this gas in its purest form at common temperatures. Copper and antimony, which so readily burn in oximuriatic gas, did not act upon the explosive gas in the cold: and when they were introduced into it, being heated, it was instantly decomposed, and its oxigen set free; and the metals burnt in the oximuriatic gas. Action of the compound on metals,

When sulphur was introduced into it, there was at first no action, but an explosion soon took place: and the peculiar smell of oximuriate of sulphur was perceived. sulphur,

Phosphorus produced a brilliant explosion, by contact with it in the cold, and there were produced phosphoric acid and solid oximuriate of phosphorus. phosphorus,

Arsenic introduced into it did not inflame; the gas was made to explode, when the metal burnt with great brilliancy in the oximuriatic gas. arsenic,

Iron wire introduced into it did not burn, till it was heated so as to produce an explosion, when it burnt with a most brilliant light in the decomposed gas. iron,

Charcoal introduced into it ignited, produced a brilliant flash of light, and burnt with a dull red light, doubtless charcoal,

vity of pure oximuriatic gas from manganese and muriatic acid is to that of common air, as 244 to 100. Taking this estimation, the specific gravity of the new gas will be about 238, and the number representing the proportion in which oximuriatic gas combines, from this estimation, will be rather higher than is stated above. oximuriatic gas,

owing

owing to its action upon the oxigen mixed with the oximuriatic gas.

nitrous gas. It produced dense red fumes when mixed with nitrous gas, and there was an absorption of volume.

muriatic acid gas. When it was mixed with muriatic acid gas, there was a gradual diminution of volume. By the application of heat the absorption was rapid, oximuriatic gas was formed, and a dew appeared on the sides of the vessel.

These experiments enable us to explain the contradictory accounts that have been given by different authors of the properties of oximuriatic gas.

Why the compound was not before observed. That the explosive compound has not been collected before is owing to the circumstance of water having been used for receiving the products from hyperoximuriate of potash, and unless the water is highly saturated with the explosive gas, nothing but oximuriatic gas is obtained; or to the circumstance of too dense an acid having been employed.

Hyperoximuriatic acid of Mr. Chevenix. This substance produces the phenomena, which Mr. Chenevix, in his able paper on oximuriatic acid, referred to the hyperoxigenised muriatic acid; and they prove the truth of his ideas respecting the possible existence of a compound of oximuriatic gas and oxigen in a separate state.

Explosions from hyperoximuriates. The explosions produced in attempts to procure the products of hyperoximuriate of potash by acids are evidently owing to the decomposition of this new and extraordinary substance.

All the facts confirm the simple nature of oximuriatic gas. All the conclusions, which I have ventured to make respecting the undecompounded nature of oximuriatic gas, are, I conceive, entirely confirmed by these new facts.

If oximuriatic gas contained oxigen, it is not easy to conceive, why oxigen should be afforded by this new compound to muriatic gas, which must already contain oxigen in intimate union. Though on the idea of muriatic acid being a compound of hidrogen and oximuriatic gas, the phenomena are such as might be expected.

If the power of bodies to burn in oximuriatic gas depended upon the presence of oxigen, they all ought to burn with much more energy in the new compound; but copper, and antimony, and mercury, and arsenic, and iron, and sulphur have

have no action upon it, till it is decomposed; and they act then according to their relative attractions on the oxigen, or on the oximuriatic gas.

There is a simple experiment, which illustrates this idea; Let a glass vessel containing brass foil be exhausted, and the new gas admitted, no action will take place; throw in a little nitrous gas, a rapid decomposition occurs, and the metal burns with great brilliancy.

Experiment.

Supposing oxigen and oximuriatic gas to belong to the same class of bodies; the attraction between them might be conceived very weak, as it is found to be, and they are easily separated from each other, and made repulsive, by a very low degree of heat.

The most vivid effects of combustion known are those produced by the condensation of oxigen or oximuriatic gas; but in this instance, a violent explosion with heat and light are produced by their separation, and expansion, a perfectly novel circumstance in chemical philosophy.

Explosion, with heat and light, accompanying expansion.

This compound destroys dry vegetable colours, but first gives them a tint of red. This and its considerable absorbability by water would incline one to adopt Mr. Chenevix's idea, that it approaches to an acid in its nature. It is probably combined with the peroxide of potassium in the hyperoximuriate.

The compound approaches to an acid.

That oximuriatic gas and oxigen combine and separate from each other with such peculiar phenomena, appears strongly in favour of the idea of their being distinct, though analogous species of matter. It is certainly possible to defend the hypothesis, that oximuriatic gas consists of oxigen united to an unknown basis; but it would be possible likewise to defend the speculation, that it contains hidrogen.

Oximuriatic gas apparently simple, and of the same nature with oxigen.

Like oxigen it has not yet been decomposed; and I sometime ago made an experiment, which, like most of the others I have brought forward, is very adverse to the idea of its containing oxigen.

I passed the solid oximuriate of phosphorus in vapour, and oxigen gas together through a green glass tube heated to redness.

Experiment.

A decomposition took place, and phosphoric acid was formed, and oximuriatic gas was expelled.

Now,

w, if oxigen existed in the oximuriate of phosphorus, is no reason why this change should take place. On lea of oximuriatic gas being undecompounded, it is explained. Oxigen is known to have a stronger ation for phosphorus than oximuriatic gas has, and conse- ly ought to expel it from this combination.

Nome

the new compound in its purest form is possessed of a t yellow green colour, it may be expedient to designate i name expressive of this circumstance, and its relation muriatic gas. As I have named that elastic fluid ne. so I venture to propose for this substance the name euchlo , as from ευ and χλωρος. The point of nomenclature not, however, inclined to dwell upon. I shall be co t any name, that may be considered as most ue by the able chemical philosophers attached to this Society.

V.

Description of a new Thrashing Machine, invented by H. P. Lee, *Esq. of Maidenhead Thicket**.

SIR,

Inducements to the invention.

I BEG leave to state to the Society of Arts &c. the following particulars, relative to my attempts to improve the thrashing machine for corn, and of my success therein.

Being largely concerned in agriculture, and having 800 acres of arable land, I found, that a thrashing machine or two became absolutely necessary for the continuance of my occupations. I accordingly erected one of the kind recommended to me; but from the complication of its structure, its being frequently out of order, and from its bad performance of the work at all times, I resolved to try to have a thrashing machine made under my own directions, more simple in its construction, and more efficacious in its operations. With this view I have continued my experiments

Its success.

for

* Trans. of the Soc. of Arts, Vol. XXVIII, p. 25. The premium of the gold medal, offered by the Society, was adjudged to Mr. Lee, for this machine.

for nearly three years, at an expense of about three hundred pounds, and have, at last, brought my machine to a degree of perfection, which is satisfactory. Many gentlemen and farmers, who have seen it and its operations, give it a decided preference to any they have seen, for the simplicity of its construction, for the cleanness of its thrashing, and for the quantity of corn thrashed by it, in proportion to the power applied.

I have no doubt but that the result of my original thoughts and experiments on this subject will be of great advantage in this highly useful agricultural implement, and I have sent a model of the machine for the Society's inspection.

I am, Sir,

Your very obedient Servant,

Maidenhead Thicket, H. P. LEE.
Dec. 27, 1809.

Certificates from Mr. Edward Green, of Bowlney, in Oxfordshire, and Mr. Thomas Michlem, of Hurley, in Berkshire, stated, that they are largely concerned in the agricultural line; that they have seen a variety of thrashing machines, but give the preference to those on Mr. Lee's principle, for the simplicity of their construction; that they highly approve of the manner, in which they perform their work; and that they consider them as calculated to thrash more corn, in proportion to the power applied, than any other they have seen. **Testimonies of its excellence.**

Certificates from William Hubbeard, of Maidenhead Thicket; Thomas Williams, of Feeres Farm, in White Waltham; Joseph Lee, of White Waltham; and Richard Silver, of Maidenham Thicket; testified, that, on the 27th of February, 1810, Mr. Lee's thrashing machine did thrash in one hour and fifty-five minutes, eight quarters and three bushels and a half of barley; that the straw was thrashed clean, and not broken; and the work was in all respects performed in a workmanlike manner. **Effect produced by it on barley,**

A certificate from James Willis, foreman to Mr. Lee, stated, that, on the 27th and 28th of February, 1810, he did **and on oats.**

...rash thirty quarters of oats with Mr. Lee's machine, ...ghway Farm, in the parish of Cookham, Berkshire.

SIR,

Thrashes wheat and barley clean without injury to the straw.

After inspecting several Thrashing Machines by a variety of makers, I saw and examined yours, at Highway Farm, and was impressed with its superiority over every other that I had seen, both on account of its simplicity and effect. I applied to Wright, your builder, who has erected one for me upon your improved principle, which effectually thrashes wheat and barley clean, without injuring the straw, and very much to my satisfaction. I have not hitherto had an opportunity of ascertaining its powers with other grain, but am happy to assure you, that I consider your improvements to constitute a material step toward perfecting an instrument of the first consequence to the agricultural interests of this kingdom, and highly deserving our warmest acknowledgments.

I have the honour to subscribe myself, Sir,

Your obliged humble Servant,

SAMUEL NICHOLLS, M. D.

Hinton House, Twyford, Berks,
March 1, 1810.

Farther testimonies.

A certificate from Mr. G. H. Crutchley, of Sunning Hill Park, Berks, dated March 3, 1810, stated, that he had seen Mr. Lee's thrashing machine at work; that it thrashed clean, and pleased him so well, that he had ordered one on the same principle.

By subsequent letters received from Dr. Nicholls and Mr. G. H. Crutchley, the above certificates were confirmed by them, with additional testimonies in favour of Mr. Lee's machines.

Account of the machine and its working.

Mr. Lee, in his attendance on the Committee appointed by the Society for the examination of the merits of his machine, stated, That his machine requires no rollers for entering the corn to be thrashed.

That

That it is about three feet diameter, and about two feet six inches in length.

That two horses are quite sufficient to work it; that from half past seven to two o'clock they will, without fatigue, thrash two loads of wheat, each of forty bushels.

That he thinks the straw is not so much broken as with other machines.

That the vanes within the cylinder turn from one hundred to one hundred and twenty times round for one round of the horses, in a space of twenty-two feet diameter.

That there are four vanes within the drum or cylinder, each vane one inch and a half thick, and enclosed to within about three inches of their exterior edges; that the drum or cylinder, within which the vanes turn, is close-fluted with wood of about an inch thick, and is in movable parts, so as to admit of being placed nearer to, or farther from, the vanes, as the corn to be thrashed may require.

That he has erected two of these machines on his estate, and has used them for three years.

A note sent to the Society by William Wright, of Henley upon Thames, Oxfordshire, the maker, states, that the price of a thrashing machine on this principle, including the horse-wheel, is forty-eight pounds, at his manufactory there. Price.

Reference to the Engraving of Mr. LEE's *Thrashing Machine, Pl.* VII, *Fig.* 4 *and* 5.

Fig. 4 and 5 are a side and end-view of the machine; A, in both figures, represents the framing of the machine; B is the shaft of a cog-wheel C, which is turned by cog-wheels, from the great horse-wheel, in the same manner as the ordinary thrashing mill; the cog-wheel C turns a small pinion D, to which it gives a rapid revolution; on the axis of the pinion, the beaters EE are fixed, and revolve with it, within a segment or drum, formed of iron plates, grooved or ribbed, parallel to the axis, as the figure represents, and connected together by wooden curbs FF, to which they are screwed. *a a* is the feeding board upon which the corn is placed to enter the machine. The end of this board is fixed very near to the four vanes, or beaters, *b b b b*; as these revolve rapidly they strike the heads of the corn upwards, with such a jerk Explanation of the plate.

as

...at out all the corn from those ears which they meet ...y; but if any escape they are drawn in, together with ...w, and rubbed round by the beaters against the in... the ribbed drum, or cylinder, F, so as to open the ...l let out the corn, though the ears come in any posi... ...atever. At H is a grating, upon which the beaters ...iver the corn, chaff, and straw all together; the two former fall through upon the ground at X, and the latter slides down on the grate; the corn is afterward to be dressed in a winnowing machine, which separates the light and heavy corn from the chaff. The curbs F are fixed by screws, which can be adjusted so as to bring the cylinder nearer, or farther from, the beaters, to adapt the machine for thrashing different kinds of grain; for it is evident, that large corn, as pease, beans, &c., must require more space to rub them in than the smaller grain, as wheat and barley. L, fig. 4, is one of the uprights of the frame which supports the bearing for the axis B of the cog-wheel; and M is an oblique brace, which strengthens the frame. N is the stage on which the man who feeds the machine stands.

VI.

Account of a Substitute for Hemp, prepared from Bean Stalks. By the Rev. JAMES HALL, *of Chesnut Walk, Walthamstow**.

Fibres in the stalk of the bean

THOUGH it has not been attended to, or, so far as I know, ever been mentioned by any one, yet it is certain, that, according to its size, every bean plant contains from 20 to 35 filaments, or fibres, running up on the outside, under a thin membrane, from the root to the very top all around, the one at each of the four corners being *rather thicker*, and stronger than the rest. It is also certain, that, next to Chinese, or sea-grass, in other words, the material with which hooks are sometimes fixed to the end of fishing lines, the filaments, or hempen particles of the bean plant,

exceedingly strong.

* Trans of the Soc. of Arts, vol. XXVII, p. 57. The silver medal was voted to Mr. Hall.

are

are among the strongest yet discovered. These, with a little beating, rubbing, and shaking, are easily separated from the strawy part, when the plant has been steeped 10 or 12 days in water: or is damp, and in a state approaching to fermentation, or what is commonly called rotting. Washing and pulling it through hackles, or iron combs, first coarse, and then finer, is necessary to the dressing of bean-hemp; and so far as I have yet discovered, the easiest way of separating the filaments from the thin membrane that surrounds them.

Method of separating them.

From carefully observing the medium number of bean-plants in a square yard, in a variety of fields on both sides the Tweed, as well as in Ireland, and multiplying them by 4840, the number of square yards in an acre, and then weighing the hemp, or filaments of a certain number of these stalks, I find, that there are at a medium about 2cwt. of hemp, or these filaments, in every acre, admirably calculated for being converted into a thousand articles, where strength and durability is of importance, as well as, with a little preparation, into paper of all kinds; even that of the most delicate texture.

An acre yields about 2 cwt.

Now since there are, at least 200000 acres of ticks, horse, and other beans planted in Great Britain and Ireland; and since, where there is not machinery for the purpose, the poor, both young and old, females as well as males, belonging to each of the 9700 parishes in England, &c. where beans are raised; might (hemp having risen of late from 60 to 120 pounds per tun), be advantageously employed in peeling, or otherwise separating these filaments from the strawy part of the plant, after the beans have been threshed out; I leave it to the feelings of the Society for the Encouragement of Arts &c. to judge of the importance of the idea held out here, not only to the poor, but to the landholders, and the community at large.

About 20000 tuns might be procured annually in the United Kingdom.

It is nearly twelve months since, by analyzing its component parts, I discovered hemp in the bean plant. I would have written to you then, Sir, on the subject, and sent a specimen, but that I was trying experiments with other plants, as I am during my leisure hours doing at present; and I wished to ascertain in what degree this species of hemp is liable to injury from different situations, and the changes

The hemp stands exposure to air,

of the atmosphere. With a view to this, I exposed ...e', nearly 12 months, to all the varieties of the air ...doors, and kept another nearly as long *constantly* under water, and find them not in the least injured. The ...fference I perceive is, that the one kept constantly u... water, namely the *whitest* of the specimens sent you, has assumed a rich silky gloss, and a much more agreeable colour than it had before.

Before it is dressed it is injured by the alternate action of air and moisture,

But though this is the case with bean-hemp *after* it is cleaned and dressed, and which, though stiff and hard when dry, is pliable and easily managed when rather damp or wet, it seems otherwise with it *previous* to its being separated from the straw. If bean-straw be kept for years under water, or quite dry, it produces I find hemp as good and fresh as at first. But, if the straw be sometimes wet, and sometimes dry, the filaments or fibres are apt to be injured. The specimen of bean-hemp accompanying this letter, in the form of oakum for caulking ships, having been long exposed to the varieties of the weather, previous to being separated from the straw, is a proof of its being considerably injured. If the straw of the bean was scattered thin on the ground, and exposed to the weather for two or three months, I have uniformly found that the hemp, or fibres, are loosened, and easily separated from the strawy part, without any other process than *merely* beating, rubbing, and shaking them, and perhaps this is the easiest way of obtaining bean-hemp; but then, from being thus exposed, and the fermentation that takes place in the strawy part, which is of a spungy nature, communicating itself to the fibres, or hemp, I find that these are generally less or more injured, though not so much so, in my opinion, as to prevent them from being excellent materials for making paper.

but is still fit for paper.

The water in which it is steeped perhaps rather beneficial than injurious to cattle.

I have also found, and the importance of the idea will, I hope, be an excuse for mentioning it here, that, though the water, in which bean-straw has been put to steep, in a few days generally acquires a black colour, a blue scum, and a peculiar taste, yet cattle drink it greedily, and seemed fattened by it. But my experiments have hitherto been on too limited a scale to be able, in a satisfactory manner, to ascertain this last circumstance. When the water, in which bean

bean straw has been put to steep, becomes fœtid, which I find it is *scarcely* more apt to become than common stagnant water, on being stirred by driving horses or cattle through it, by a stick, or in any other way set in motion, (as is the case with all putrid water, even the ocean itself,) the fetid particles fly off, and the effluvia die away.

When straw is to be steeped for bean hemp, the beans are to be thrashed in a mill: the beans should be put to the mill, not at *right angles*, but on a *parallel*, or nearly so with the rollers, else the straw, particularly if the beans are very dry, is apt to be much cut. If the straw is *not* to be steeped, on putting the beans to be thrashed at right angles, or nearly so, with the rollers of the mill, a certain proportion of the fibres, or hemp, may easily be got from the straw, these being in general not so much cut as the straw; but often found torn off and hanging about it like fine sewing threads. The hemp thus taken off, though its lying under water for months would do it no harm, requires only to be steeped a few minutes, drawn through a hackle, and washed, previous to its being laid up for use. If the hemp or fibres, collected in this way (which is a fine light business for children, and such as are not able for hard work, and which requires no ingenuity,) are intended only for making paper, they require neither steeping nor hacklings, but only to be put into parcels and kept dry till sent off to the manufacturer.

Mode of thrashing the beans.

The straw of beans contains a saccharine juice, and is highly nutritive, perhaps more so than any other; and like clover, the prunings of the vine, the loppings of the fig-tree, &c., produces a *rich* infusion, and uncommonly fine table-beer, as well as an *excellent* spirit by distillation. It is the hemp, or fibres, that prevents cattle from eating it. These, like hairs in human food, make cattle dislike it. The collecting of it therefore should never be neglected, nor the boys and girls in workhouses and other places be permitted to be idle, while business of this kind would evidently tend both to their own and their employers' advantage.

Bean straw nutritive, and capable of producing a fermentable liquor.

It is a fact, that about the generality of mills for beating and dressing hemp and flax, a large proportion, in some inland places both of Great Britain and Ireland amounting nearly

Refuse of hemp and flax a valuable material for paper.

y to one half of what is carried thither, is either left to rot, under the name of refuse, or thrown away as use, because too rough and short for being spun and rted into cloth. Now, from the experiments I have , and caused to be tried, I have uniformly found, that, ugh too rough and short for being converted into cloth, en of the coarsest kind, the refuse of hemp and flax, on beat and shaken, so as to separate the strawy from e stringy particles, which can be done in a few minutes by a mill or hand-labour, as is most convenient, becomes thereby as soft and pliable, and as useful for making-paper, as the longest, and what is reckoned the most valuable part of the plant, after it has been converted into cloth and worn for years.

May be made very white.

In its natural state, it is true, the refuse of hemp and flax is generally of a brown and somewhat dark colour. But what of that? By the application of muriatic acid, oil of vitriol, or other cheap ingredient, well known to the chemists, as well as to every bleacher, such refuse, without being *in the least* injured for making paper, can, in a few hours, if necessary, be made as white as the finest cambric.

Number of newspapers published in London.

There are, at a medium, published in London, every morning, 16000 newspapers, and every evening about 14000. Of those published every other day there are about 10000. The Sunday's newspapers amount to about 25000; and there are *nearly* 20000 other weekly papers, making in all the enormous sum of 245000 per week. At a medium 20 newspapers are equal to one pound—hence the whole amount to about 3 tuns per week, or 260 tuns per annum. But though this, perhaps, is not one half of the paper expended in London on periodical publications, and what may be called fugacious literature; and not one fourth part of what is otherwise consumed in printing-houses in the country at large; yet there are materials enough in the refuse of the hemp and flax raised in Britain and Ireland for all this and much more.

These consume 260 tuns of paper annually.

Hopbines contain hemp.

Nor is this all, for as the bine or straw of hops, a circumstance well known to the Society, contains an excellent hemp for making many articles, so also will it prove a most excellent material for making all kinds of paper. And it is a fact,

a fact, that, were even the one half of the bine of hops raised in the counties of Kent, Sussex, and Worcester, instead of being thrown away, or burnt, after the hops are picked, as is commonly done, steeped for ten or twelve days in water, and beaten in the same way as is done with hemp and flax, independent of what might be got from bean-hemp, and a variety of articles well-known to the Society, there would be found annually materials enough for three times the quantity of paper used in the British dominions.

I have the honour to be,

with much respect,

Sir,

Your most humble servant,

Streatham, Jan. 9, 1809. JAMES HALL.

Certificates of the Truth of the foregoing Statement.

We, the undersigned, do hereby certify, that the specimens of hemp enclosed and sealed up by us, addressed to Dr. Taylor, Secretary to the Society for the Encouragement of Arts, Manufactures, and Commerce, Adelphi, Strand, are the produce of common bean straw:—That we never saw or heard of bean hemp till lately; when the Rev. James Hall, who resides here at present, was trying experiments respecting it at Mr. Adams's farm, Mount Nod, and other parts of this parish:—That, in the present obstructed state of commerce with the Continent, it appears to us the discovery of bean hemp may be extremely useful to the manufacture of canvas, ropes, paper, &c.;—And that, as it affords a new and important prospect of employment for the poor, we think Mr. Hall, the discoverer, is deserving of the approbation of the public. We shall only add, that as the Society for the Encouragement of Arts, Manufactures, and Commerce, have contributed so often in a high degree to the exertion of genius, the improvement of the arts, and the public good, we have no doubt but they will not only take the proper steps to prosecute the discovery and encourage the manufacture of bean hemp, but also, by some mark of their favour, show their approbation of Mr. Hall's merit in the

Testimonies of the use of hemp from beanhaulm.

…very he has made, as well as of his high public spi… liberality in communicating the discovery to the …without reserve.

WILLIAM ADAMS, Mount Nod,
EDWARD BULLOCK, Curate.
Streatham, Surry, Jan. 9, 1809. WM. GARDNER, Surgeon.

These are to certify to the Secretary of the Society for the Encouragement of Arts &c., London, and all whom it may concern, that having seen (at first to our astonishment) the Rev. James Hall, who has resided here for some time past, procuring hemp from common bean straw, steeped some days in water, we steeped some also, and easily got hemp from it; there being no mystery in the matter more than *merely* steeping the straw, peeling off the hemp, and then washing and cleaning it, by pulling it through a hackle or comb.

It answers extremely well for sewing shoes.

These are also to certify, that having tried bean hemp, and found it to take both wax and resin, we have sewed with it, and find the fibres of which it consists in general so strong, that the leather never failed to give way sooner than the seam. We have only to add, that as hemp has of late become uncommonly dear, while much of it is bad, we anxiously wish the prosecution of the discovery, and the appearance of bean hemp in the market; and shall, so soon as we hear of its being spun and on sale, be among the first to purchase and use it.

JOHN HOUNE, Shoemaker.
THOMAS ALFORD, Shoemaker.

Letter from Mr. HUME, *of Long Acre, to the Reverend James Hall.*

SIR,

It bears bleaching extremely well.

I enclose a specimen of the bean filaments, or thread, which have been submitted to the bleaching process. The texture and strength seem not in the least to have been impaired, but retain the primitive tenacity; and I am persuaded this substance will prove an excellent substitute for hemp and flax, for the manufacture of various kinds of paper,

per, cordage, and other materials. I did not find more difficulty in accomplishing the bleaching of this than in other vegetables which I have occasionally tried, and I believe this article is susceptible of a still greater degree of whiteness.

I remain, Sir,

Your very obedient servant,

Long Acre, Feb. 24, 1807. JOS. HUME.

Letter from Mr. H. Davy to the Rev. James Hall.

SIR,

I shall enclose in this paper a small quantity of the bean fibre, rendered as white as possible by chemical means.

It seems to bear bleaching very well, and, as to chemical properties, differs very little from hemp.

The question, whether it is likely to be of useful application, is a *mechanical* one, and must be solved by experiments on its comparative strength.

I am, Sir,

Your obedient humble servant,

H. DAVY.

VII.

A Chemical Analysis of Sodalite, a new Mineral from Greenland. By THOMAS THOMSON, *M. D. F. R. S. E., Fellow of the Imperial Chirurgo-Medical Academy of Petersburgh* *.

THE mineral, to which I have given the name of *sodalite*, was also put into my hands by Mr. Allan †. In the Greenland collection which he purchased, there were several specimens of a rock, obviously primitive. In the composition of these the substance of which I am about to treat formed a constituent,

Sodalite, a new mineral, in the composition of a primitive rock.

* From the Transactions of the Royal Society of Edinburgh.

† See p. 47.

[...]stituent, and, at first appearance, was taken for felspar, [...]hich it bears a very striking resemblance.

Compo[...] of this [...]. [...]his rock is composed of no less than five different fossils, [...]mely, garnet, hornblende, augite, and two others, which form the paste of the mass. These are evidently different minerals; but in some specimens are so intimately blended, that it required the skill of Count Bournon to make the discrimination, and ascertain their real nature. Even this distinguished mineralogist was at first deceived by the external aspect, and considered the paste as common lamellated felspar, of a greenish colour. But a peculiarity, which presented itself to Mr. Allan in one of the minerals, induced him to call the attention of Count Bournon more particularly to its construction.

Crystals of sahlite, On a closer examination of the mineral, Mr. de Bournon found, that some small fragments, which he had detached, presented rectangular prisms, terminated by planes, measuring, with the sides of the prism, 110° and 70° or nearly so,—a form which belongs to a rare mineral, known by the name of sahlite, from Sweden. He farther observed, intermixed along with this, another mineral; and after some trouble, succeeded in detaching a mass, presenting a regular rhomboidal dodecahedron. It was to this form that Mr. Allan had previously requested his attention.

and of another mineral

resembling the Swedish natrolite of Dr. Wollaston.

Some time before this investigation, Mr. de Bournon had examined a mineral from Sweden, of a lamellated structure, and a greenish colour, which, he found, indicated the same form. From this circumstance, together with some external resemblance, which struck him, he was induced to conclude, that our mineral was a variety of that substance.

To that substance the name of Swedish *natrolite* had been given, in consequence of the investigation of Dr. Wollaston, who found that it contained a large proportion of soda.

Natrolite of Klaproth very different.

There are few minerals, however, that are so totally distinct in their external characters as the natrolite of Klaproth, and the substance we are now treating of. The mineral examined by Klaproth occurs at Roegau *, on the Lake of Constance, in porphyry-slate, coating the sides of veins and cavities

* It has been observed also by Professor Jameson, in the flœtz trap rocks behind Burntisland.

ties in a mamellated form, the texture of which is compact, fibrous, and radiated; the colour pale yellow, in some places passing into white, and marked with brown zones. Hitherto it had never been found in a state sufficiently perfect to afford any indications of form. Lately, however, Mr. de Bournon was so fortunate as to procure some of it, presenting very delicate needleform crystals, which, by means of a strong magnifier, he was able to ascertain presented flat rectangular prisms, terminated by planes, which, he thought, might form angles of 60° and 120 with the sides of the prism. With this neither our mineral nor the Swedish can have any connection, farther than some analogy which may exist in their composition.

Concerning the Swedish mineral I have not been able to obtain much satisfactory information. There is a specimen of it in Mr. Allan's cabinet, which he received directly from Sweden, sent by a gentleman who had just before been in London, and was well acquainted with the collections of that city, from which it is inferred, that the specimen in question is the same as that examined by Count Bournon and Dr. Wollaston.

Werner has lately admitted into his system a new mineral species, which he distinguishes by the name of Fettstein. Of this I have seen two descriptions; one by Haüy, in his Tableau Comparatif, published last year; and another by Count Dunin Borkowski, published in the 69th volume of the Journal de Physique, and translated in Nicholson's Journal, (Vol. XXVI, p. 384). The specimen, called Swedish natrolite, in Mr. Allan's possession, agrees with these descriptions in every particular, excepting that its specific gravity is a little higher. Borkowski states the specific gravity of fettstein at 2·563; Haüy at 2·6138; while I found the specific gravity of Mr. Allan's specimen to be 2·779, and, when in small fragments, to be as high as 2·790. This very near agreement in the properties of the Swedish natrolite with the characters of the fettstein leads me to suppose it the substance, to which Werner has given that name. This opinion is strengthened, by a fact mentioned by Haüy, that fettstein had been at first considered as a variety of wernerite. For the specimen sent to Mr. Allan, under the name of compact wernerite,

Fettstein

apparently the same with the Swedish natrolite.

ite, is obviously the very same with the supposed na- of Sweden. Now, if this identity be admitted, it will , that our mineral constitutes a species apart. It bears, , a considerable resemblance to it; but neither the line form, nor the constituents of fettstein, as stated by are similar to those of the mineral to which I have the name of sodalite. The constituents of fettstein, rtained by Vauquelin, are as follows:

Constituents of fettstein.

Silica	44·00
Alumina	34·00
Oxide of iron	4·00
[illegible]	0·12
[illegible]	16·50
[illegible]	1·38
	100·00

Description of sodalite.

Sodalite, as has been already mentioned, occurs in a primitive rock, mixed with sahlite, augite*, hornblende, and garnet†.

It occurs massive; and crystallised, in rhomboidal dodecahedrons, which, in some cases, are lengthened, forming six-sided prisms, terminated by trihedral pyramids.

Its colour is intermediate between celandine and mountain green, varying in intensity in different specimens. In some cases it seems intimately mixed with particles of sahlite, which doubtless modify the colour.

External lustre glimmering, internal shining, in one direction vitreous, in another resinous.

Fracture foliated, with at least a double cleavage; cross fracture conchoidal.

Fragments indeterminate; usually sharp-edged.

Translucent.

Hardness equal to that of felspar. Iron scratches it with difficulty.

* This situation of the augite deserves attention. Hitherto it has been, with a few exceptions, found only in flœtz-trap rocks.

† The particular colour and appearance of this garnet shows, that the rock came from Greenland: for similar garnet has never been observed, except in specimens from Greenland.

Brittle.

Brittle.

Easily frangible.

Specific gravity, at the temperature of 60°, 2·378. The specimen was not absolutely free from sahlite.

When heated to redness, does not decrepitate, nor fall to powder, but becomes dark gray, and assumes very nearly the appearance of the Swedish natrolite of Mr. Allan, which I consider as fettstein. If any particles of sahlite be mixed with it, they become very conspicuous, by acquiring a white colour, and the opacity and appearance of chalk. The loss of weight was 2·1 per cent. I was not able to melt it before the blow-pipe.

1. A hundred grains of the mineral, reduced to a fine powder, were mixed with 200 grains of pure soda, and exposed for an hour to a strong red heat, in a platinum crucible. The mixture melted, and assumed, when cold, a beautiful grass-green colour. When softened with water, the portion adhering to the sides of the crucible acquired a fine brownish-yellow. Nitric acid being poured upon it, a complete solution was obtained. Chemical analysis.

2. Suspecting, from the appearance which the fused mass assumed, that it might contain chromium, I neutralised the solution, as nearly as possible, with ammonia, and then poured into it a recently prepared nitrate of mercury. A white precipitate fell, which being dried, and exposed to a heat rather under redness, was all dissipated, except a small portion of gray matter, not weighing quite 0·1 grain. This matter was insoluble in acids, but became white. With potash it fused into a colourless glass. Hence I consider it as silica. Silex. This experiment shows, that no chromium was present. I was at a loss to account for the precipitate thrown down by the nitrate of mercury. But Mr. Allan having shown me a letter from Ekeberg, in which he mentions, that he had detected muriatic acid in sodalite, it was easy to see that the white precipitate was calomel. The white powder weighed 26 grains, indicating, according to the analysis of Chenevix, about three grains of muriatic acid. Muriatic acid.

3. The solution, thus freed from muriatic acid, being concentrated by evaporation, gelatinised. It was evaporated nearly to dryness; the dry mass digested in hot water aci- Silex.

 dulated

dulated with nitric acid, and poured upon the filter. The powder retained upon the filter was washed, dried, and heated to redness. It weighed 37·2 grains, and was silica.

4. The liquor which had passed through the filter was supersaturated with carbonate of potash, and the copious white precipitate which fell collected by the filter, and boiled while yet moist in potash-lie. The bulk diminished greatly, and the undissolved portion assumed a black colour, owing to some oxide of mercury with which it was contaminated.

5. The potash-lie being passed through the filter, to free it from the undissolved matter, was mixed with a sufficient quantity of sal-ammoniac. A copious white precipitate fell, which being collected, washed, dried, and heated to redness, weighed 27·7 grains. This powder, being digested in sulphuric acid, dissolved, except 0·22 of a grain of silica. Sulphate of potash being added, and the solution set aside, it yielded

Alumine. alum crystals to the very last drop. Hence the 27·48 grains of dissolved powder were alumina.

6. The black residue, which the potash-lie had not taken up, was dissolved in diluted sulphuric acid. The solution being evaporated to dryness, and the residue digested in hot

Lime. water, a white soft powder remained, which, heated to redness, weighed 3·6 grains, and was sulphate of lime, equivalent to about 2 grains of lime.

7. The liquid from which the sulphate of lime was separated, being exactly neutralised by ammonia, succinate of

Oxide of iron. ammonia was dropped in; a brownish red precipitate fell, which, being heated to redness in a covered crucible, weighed one grain, and was black oxide of iron.

8. The residual liquor being now examined by different reagents, nothing farther could be precipitated from it.

9. The liquid (No. 4.) from which the alumina, lime, and iron had been separated by carbonate of potash, being boiled for some time, let fall a small quantity of yellow-coloured matter. This matter being digested in diluted sulphuric acid, partly dissolved, with effervescence; but a portion remained undissolved, weighing 1 grain. It was insoluble in acids, and with potash melted into a colourless

Silex. glass. It was therefore silica. The sulphuric acid solution being

being evaporated to dryness, left a residue, which possessed the properties of sulphate of lime, and which weighed 1·2 grains, equivalent to about 0·7 of a grain of lime. Lime.

10. The constituents obtained by the preceding analysis being obviously defective, it remained to examine whether the mineral, according to the conjecture of Bournon, contained an alkali. For this purpose, 100 grains of it, reduced to a fine powder, and mixed with 500 grains of nitrate of barytes, were exposed for an hour to a red heat, in a porcelain crucible. The fused mass was softened with water, and treated with muriatic acid. The whole dissolved, except 25 grains of a white powder, which proved on examination to be silica. The muriatic acid solution was mixed with sulphuric acid, evaporated to dryness; the residue, digested in hot water, and filtered, to separate the sulphate of barytes. The liquid was now mixed with an excess of carbonate of ammonia, boiled for an instant or two, and then filtered, to separate the earth and iron precipitated by the ammonia. The liquid was evaporated to dryness, and the dry mass obtained exposed to a red heat in a silver crucible. The residue was dissolved in water, and exposed in the open air to spontaneous evaporation. The whole gradually shot into regular crystals of sulphate of soda. This salt, being exposed to a strong red heat, weighed 50 grains, indicating, according to Berthollet's late analysis, 23·5 grains of pure soda. It deserves to be mentioned, that during this process the silver crucible was acted on, and a small portion of it was afterward found among the sulphate of soda. This portion was separated before the sulphate of soda was weighed.

Analysed in a different way.

Soda.

The preceding analysis gives us the constituents of sodalite as follows:

Constituents of sodalite.

Silica,	38·52
Alumina,	27·48
Lime,	2·70
Oxide of iron,	1·00
Soda,	23·50
Muriatic acid,	3·00
Volatile matter,	2·10
Loss,	1·70
	100·00

Mr.

Analysis by Mr. Ekeberg.

Mr. Allan sent a specimen of this mineral to Mr. Ekeberg, who analysed it in the course of last summer. The constituents which he obtained, as he states them in a letter to Mr. Allan, are as follows:

Silica,	36·
Alumina,	32·
Soda,	25.
Muriatic acid,	6·75
Oxide of iron,	0·25
	100·00

This result does not differ much from mine. The quantity of muriatic acid is much greater than mine. The lime and the volatile matter, which I obtained, escaped his notice altogether. If we were to add them to the alumina, it would make the two analyses almost the fame. No mineral has hitherto been found containing nearly so much *soda* as this. Hence the reason of the name by which I have distinguished it.

VIII.

Account of a Primitive Gypsum. By Mr. DAUBUISSON, *Mine Engineer**.

Stratum of primitive gypsum.

IN a visit I have just made to the mine of Cogne, I had an opportunity of observing a mineralogical fact, that may be thought not uninteresting, the existence of a stratum of primitive gypsum, intercalated in the mass of the Upper Alps. Mineralogists have hitherto noticed only a single instance of such gypsum, discovered by Mr. Friesleben at the southern foot of St. Gothard in a micaceous schist; and some doubts have been started respecting the period assigned to the formation of this rock. I trust the particulars I shall relate respecting the situation of that at Cogne will evince the existence of really primitive gypsums; accordingly

Only one previous instance,

and this doubted.

* Journal des Mines, vol. XXII, p. 161.

ingly I shall begin with a few words on the mineral constitution of the country around.

The southern declivity of the Alps, from Mount Blanc to Mount Rose, belongs almost wholly to the micaceous schist formation. Here, as in other places, this schist frequently includes strata of primitive limestone, serpentine, chlorite, oxidulated iron, &c. Sometimes it passes into argillaceous schist, as at the col de l'Allée Blanche for instance; but still more frequently into gneiss and granite. Southern declivity of the Alps.

About 15000 met. [9 miles] south of the town of Aoste, and to the east of the village of Cogne, which is at its foot, rises a mountain, that forms part of the chain separating the valley of Cogne from that of Fenis. It is terminated by a sharp ridge at least 700 met. [765 yds.] above the bottom of the valley. Its absolute height appears to me nearly to equal that of the passage of the Great St. Bernard, or 2400 met. [2623 yds.] above the level of the sea. It probably rests on the granite, that shows itself on the surface 2 or 3 kilom, [10 or 15 furl.] to the north. It is composed of micaceous schist, in strata very slightly inclined, so that they may be considered in general as horizontal. In its upper part the micaceous schist becomes loaded with limestone, so that in some little places it ends with being nothing but a white granular limestone containing merely a few spangles of mica. It includes also considerable strata of serpentine, in one of which is the celebrated iron mine of Cogne*. Mountain near Cogne.

The stratum of gypsum is found 20 met. [22 yds.] below the highest point of the ridge. It is exposed only to the length of 7 or 8 met. and 1 met. thick. Throughout the rest of its extent it is concealed by the numerous fragments of stone, that have fallen from the summit, and cover the sides of the mountain in this part: so that I can say nothing Stratum of gypsum.

* This mine, perhaps the richest in the world, exhibits the appearance of an iron quarry, which is worked in open day. The ore is oxidulated iron, in some places pure. It is in very small grains, and sometimes wholly compact. It forms a mass, that appeared to me to be a very short and thick bed. Where it is worked it is more than 25 met. [27 yds] thick. Rich iron mine.

of

of its length, thickness, or the circumstances of its superposition. However at more than 50 met. beyond the place where it has been laid open for working I have seen indications of its existence. Its thickness cannot be great, for the rock appears in its natural position a few yards below the place where it is worked. The rock at this place, as well as above the stratum, is a micaceous and calcareous schist, of a deep gray, with plane laminæ, traversed by numerous filaments of calcareous spar, and including some veins and nodules of quartz. In getting out the gypsum the workmen have advanced about two yards under the schist, so that this rock forms a projecting roof, under which they work. In this place we see in the most distinct manner, that the schist overlies the gypsum: both are stratified: their strata are perfectly parallel, and dip only a few degrees to the south-east. The strata of gypsum are a few centim. [the cent. is near 4 lines] thick, and frequently separated from each other by a greenish talcy coat.

The gypsum described.

This gypsum is of a fine white colour, with sometimes a slight rosy tinge. Its grain is very fine crystalline, similar to that of the beautiful Carrara marble. It is very translucid, and very soft. If pieces of any size, and exempt from fissures, could be got from the quarry, it would form a very fine alabaster. It is used however for building, and makes good plaster.

Talc contained in it.

It contains a great deal of talc in detached particles, generally lenticular, and varying in size from that of a lentil to that of a walnut. These almost always lie flat, and arranged in lines parallel to each other, and to the stratification. Their colour is a very pleasing green. Sometimes the laminæ of talc are so close together as to produce a kind of steatite; sometimes they are very narrow, resembling fibres, and forming together little masses, exhibiting a pleasing variety of fibrous talc. Pretty frequently these fibres are disseminated in small groupes through the gypsum, are of a delicate light green, and might be taken at first sight for amianthus, of which they have all the appearance. Martial pyrites also is seen in the gypsum, and particularly in the small masses of talc. It is sometimes in rounded grains, sometimes in little striated cubes.

What

What I have said, particularly on the parallel stratification of the gypsum and micaceous schist, as well as on the presence of the talcy or steatitic matter in these two mineral masses, evidently shows, that they are of the same formation, that is, formed at the same period. The nearly horizontal position of the strata from the foot of the mountain to its summit; the identity of the rock, that forms the roof and the wall of the strata of gypsum, &c.; all militate against the idea of a transposition, that might have covered a secondary gypsum, deposited on the mountain subsequent to its formation, with a block of schist. Here the gypsum is really a component part of the mountain; it is one of the courses, that form the building; and it has ever been placed before several others, those that are at the summit. Now the mountain of Cogne itself makes part of that portion of the Alps, *Grandi Alpi* of the Italians, which extends from Mount Blanc to Mount Rose: and it is of the same nature, as we may be satisfied by reading what Saussure has said of that country in his journey to Mount Cervin. Thus we have a gypsum of the same formation with those lofty mountains, which have always been considered as primitive, or anterior to the existence of organized beings, and which every thing still indicates to be so.

Of the same date with the other Alpine rocks.

IX.

Farther Observations on the Fructification of the Firs. In a Letter from Mrs. Agnes Ibbetson.

To Mr. NICHOLSON.

SIR,

MY endeavour to contract my subject has made me leave out much respecting the firs, I think of high consequence to them: I shall therefore add this short letter respecting the cones, that I may not be misunderstood.

Seeds of the firs

I have said that the seed of the fir is not impregnated the first year, and this is certainly *true* with respect to the *pines*, and all those *firs*, the male flower or catkin of which so much

not impregnated the first year.

precedes

precedes the female cone, as to disappear wholly before the pistil is scarcely visible. The Scotch fir will serve as a proper example. The female cone for the present year came out in June, 1811. In May, 1811, all the powder of the stamens had disappeared; besides that the cone shows no seed till full three months after its first coming, of course these seeds could not be impregnated. Next year, May 1812, the cone will show (by many outward signs) that the seed is ready to receive the line of life; the pistils in the cone will be elongated; the drops ready to be saturated with the powder of the stamen as soon as it is fit. The pistil is then in the exact situation in which I drew it in my last letter; and the impregnating and nourishing vessels distended in a manner they never are but at this time. As soon as the drops are saturated, the pistil draws in, and all is complete for the year, except that the cones continue to increase and alter their form by degrees. The following year, March 1813, they will begin to swell about the points, and in a few months to open; and this is the time the cones in England are obliged to be gathered, or they are very apt to shed their seed. Still they are *far too much attached* to their stalks; and are often greatly hurt by this early plucking. I believe it will be acknowledged that *now*, as well as in Evelyn's time, the seed of all pines are *far better* coming from abroad, than that shed in our own country: and the reason is plain: they are able (from the shade either of the mountain or the forest where they grow) or from the northern climate, to remain on the trees *till ripe*; so that they are not gathered till the fourth or fifth season. That is, if appearing in 1811, they are not sent to this country till 1814 or 1815: by which means, the integuments will be completely loosened from the tree, (not torn as ours are), their seed will be perfected, and full of moisture; and if in taking out the seed *some injury* is done to the vessels, it signifies *little*, as the process is completed: but in ours, when the seed is but half formed, to injure the vessels is to stop the completion of the seed, and this is the exact difference between the two sorts when examined: the work of Nature is finished in the foreign seed; in ours it is not perfected. No person, who is not a

Scotch fir.

Foreign seed of pines best.

dissector,

dissector, *can conceive* the mischief done to such seeds, in thus tearing the chief vessels in the cone.

In the pines it is most easy to know the cone of the year for several years back, as they are always found on the year's shoot to which they belong. By tracing each shoot the tree has made a few years back, this will be found *never to vary*. The first year's cone is *white* and close; the second year's is green and close; the third brown; the fourth brown and open; and each falls back one year's shoot. Cones of the pine of different years growth described.

With respect to the larch, it is very different; in *habit*, *nature*, and *appearance*, no trees can differ more. The larch gives her fruit in an irregular manner, equally on the old as on the new wood. It has also its female catkin appearing before the male, and so much preceding it, that the seed is ready for impregnation, ere the powder of the stamen is ripe: this is easily known, by dissecting the cone of the *last year*, and comparing it with the present. The distended vessels, which are most observable at the back; the opening of the nourishing vessels, and above all the bubble, if watched for in April or May, prove this early impregnation. I doubt not it is also the case with the cedar of Lebanon, though the cones of *this tree* hang afterwards till the fourth or fifth season, as their appearance testifies. The quantity of tannin (or of that juice which appears to contain it), is excessive, and seems nearly as much as is contained in the bark; for the cone part (when the seeds are taken out), if magnified, shows nothing but bladders of this juice. Fructification of the larch. Cedar of Lebanon. Tannin.

As to the cypresses, and those I have ventured to rank with them, (like the pines), they are too late in the year for impregnation; beside that the seed is not formed, or the cone opened, till late in the autumn. Cypresses.

Since I have turned my mind to remark the quantity of tannin found in trees, I have observed how much more is found in many, than in the *oak*. In the betula alnus of this country there is certainly a very great quantity, though not so much as in the firs. The men's hands who bark it are always so stained, that they find it very difficult to obliterate it; which is not the case when they strip the oak. After studying the firs, my hands were so stained, I had great trouble to take it from them; and yet the *guarding* Quantity of tannin in different trees.

wood

wood of the firs is of a beautiful yellow white, till exposed to the air, when it becomes a deep brick red.

I am, Sir,

Your obliged servant,

AGNES IBBETSON.

X.

Description of a Screw adjusting Plough, invented by Mr. THOMAS BALLS, *of Saxlingham, near Holt, Norfolk**.

SIR,

Plough on a new construction.

I HUMBLY offer, for the inspection of the Society, the model of a plough, constructed upon a principle on which I have made several.

Sir Jacob Astley, Bart., has seen two at work on my farm, which I have constantly used, in different kinds of ploughing, for three years, and which, excepting in the share, have not cost me a shilling in repairs. Sir Jacob has ordered one to be made; and he being desirous, that the plough should be more generally known, expressed a wish that I would send a model to the Society. If its mechanical principle proves to be of real utility to agriculturists, and superior to the ploughs in general use, I shall be highly gratified in my endeavours to promote the liberal views of the Society.

I am, Sir,

Your most obedient humble Servant,

Saxlingham, April 5, 1810. THOMAS BALLS.

Certificate from Sir JACOB HENRY ASTLEY, Bart.

DEAR SIR,

Testimony of its utility.

I have seen Mr. Ball's plough worked against the common Norfolk plough, and find it much superior. It laid the furrow much better, more equal, and with much less draught

* Trans. of the Soc. of Arts, Vol. XXVIII, p. 45. The silver medal was voted to Mr. Balls for this invention.

to

to the horses, and has not wanted the usual repairs, which the common ploughs are subject to. I make this observation from having had one in use for more than a year; and I find this plough much approved of by the farmers in this neighbourhood.

I remain, Dear Sir,

Your most obedient Servant,

Milton-Constable, May 3, 1810. J. H. ASTLEY.

SIR,

The enclosed certificates will, I hope, be satisfactory to the Society respecting my plough. It is a material improvement over the wheel-plough in common use in Norfolk, as it works with greater ease to the horses, on account of the line of draught being on a line with the angle of the horse's shoulders. It lays the furrow-slice particularly level, and cuts an even bottom-furrow. It is less liable to wear, on account of having less friction on the ground irons. It is particularly well calculated for breaking up stiff old land, and less liable to be put out of order than any plough generally used. By the adjusting screw, the furrow may be set from one to nine inches in depth, and secured by a lock to any of those intermediate depths with the greatest exactness. It may be easily converted into a swing-plough, by disengaging the axle-tree and wheels. Its beam may be made particularly light, on account of the line of draught lying so near the heel. I beg leave to inform the Society, that the Earl of Thanet, in the year 1807, ordered two of these ploughs, and in 1809 six more of them. Mr. Burroughs, of Weasenham, intends to have all his ploughs on this plan; also Mr. Wall, of Bayfield-lodge; Mr. Cobon, of Letheringsett, will have two ploughs; and the Rev. T. Munnings has given orders for some to be made.

Advantages of this plough.

Its use adopted by several.

If I had not been so limited in time, I could have sent you many more certificates.

I am, Sir,

Your most obedient humble Servant,

THOMAS BALLS.

Saxlingham, May 6, 1810.

Certificates

Farther testimonials.

Certificates were received from the following persons: viz. Mr. Robert Wright, of Great Snoring, stating, that he has three of Mr. Ball's ploughs, which he conceives to be much superior to the common plough, both in the execution of the work and easiness of draught.

Mr. Mark Barret, farming steward to Sir George Chad, stating that he has three of Mr. Ball's ploughs; that they are the best he has ever made use of, and answer every purpose, both as a swing and wheel-plough.

Mr. Thomas Hurrell, of Saxlingham, stating his opinion, that Mr. Ball's plough will come into extensive use, being an excellent plough for general purposes.

Mr. Henry May Waller, farming steward to Sir Jacob Henry Astley, Bart., stating, that he has two of Mr. Ball's ploughs in constant use; that he thinks them well calculated for strong work; and that they may be converted into a swing-plough, by disengaging the wheels.

Reference to the Drawing of Mr. Ball's *Plough, Fig.* 1, *Pl.* VIII.

Description of the plough.

A is the beam of the plough carrying the coulter B, share D, and handle E; F is the mould board; the draught of the plough is taken by two iron rods G, connected at one end with a hook *a* in the beam A; and at the other with an iron bridle H by a swivel-bolt; this iron bridle has several notches to receive the draught-chain I, by means of which the point of traction is adjusted sideways; the adjustment for height, and in which the improvement consists, is made by an iron frame K, at the top of which a nut is placed acting upon a screw *d* fixed into the beam A; the axletree *e* of the wheels *ff* is connected with the iron rods G, by a single bolt or pivot projecting from the end of them, which passes through the axletree; by these means the wheels always apply themselves to the inequalities of the ground without influencing the motion of the plough. The nut of the screw *d*, being turned, raises or lowers the iron rods G, and elevates or depresses the point of traction, so that the plough will cut a greater or less depth of furrow.

XI.

[redacted]

XI.

An improved Implement for extirpating Docks and Thistles; by Mr. J. Baker, *of West-Coker, near Yeovil, in Somersetshire*.*

SIR,

I HAVE sent to the Society an implement of my invention for destroying thistles and docks, which are two very injurious weeds to agriculturists.

Implement for destroying thistles and docks.

The implement is so contrived, that, if the root breaks in the claw, in attempting to draw it, you may, by turning the instrument, cut the root so far below the turf as to prevent its growth.

I am, Sir,
Your obedient Servant,
JOHN BAKER.

West-Croker, Oct. 31,
1809.

Certificate.

We do hereby testify, that the instrument made by Mr. John Baker for destroying docks and thistles has been used to great advantage, and is likely to come into general use.—Edward Guppy, Nathaniel Bartlett, Thomas Sandford, Edward Penny.

Testimonies of its utility.

Description of the Implement.

Fig. 2 of Pl. VIII represents Mr. Baker's thistle-extirpator. A is the handle; B the claws, between which the thistle is received; the curved iron C is the fulcrum, over which the purchase to extract the weed is obtained; D is an iron rod, or bar, upon which the foot is placed to thrust the claws into the ground. In case the root of the weed breaks in endeavouring to extract it, the curved blade E, which has a sharp end like a chissel, is thrust into the ground to cut off the root of the thistle some inches below the surface, and prevent its vegetation.

The implement described.

* Trans. of Soc. of Arts, vol. XXVIII, p. 50. The gold medal was voted to Mr. Baker for this invention.

Description

XII.

Description of a Pair of Expanding Harrows, applicable both for cleaning foul Land, and harrowing in Seeds. By Mr. William Jeffery, *of Cotton-End, Northampton**.

SIR,

New invented harrows.

I HAVE sent, for the Society's inspection, a model of a pair of harrows of my own invention, made to a scale of one inch and a half to a foot, and which are allowed to be a great improvement in these implements.

The improvement stated.

The improvement consists in their power of contraction or expansion, so as to cover an extent of land from five feet to ten feet; their teeth may be set at twelve different distances between them, and their tracks will always be at equal distances, according to the state of the land; they will either serve for harrowing in seeds, or cleaning foul land.

For cleaning foul land this harrow far exceeds any other yet made; for in such land the teeth ought to be at a greater distance in the first harrowing, and at the subsequent harrowings to be brought nearer together by degrees, till the teeth are brought very near together by contracting them. One pair of my harrows answer the purposes of three or more pairs made upon the old construction with fixed teeth.

My harrows are so constructed as to be contracted, or expanded, in two or three minutes; and the teeth, which are thirty-four in number, set at any equal distances required, having only two screws to confine them. This implement is more durable than other harrows, as there are no mortices or tenons in them to weaken the wood-work, or admit the rain. They are put together with iron nuts and screws.

They are also easier conveyed from field to field than other harrows, and when not in use will fold up in a small compass. I hope they will meet the Society's approbation, and be rewarded according to their merit.

I remain, Sir,

Your humble Servant,

WILLIAM JEFFERY.

Cotton-End, Northampton,
June 8, 1807.

* Trans. of Soc. of Arts, vol. XXVIII, p. 51. The silver medal was voted to Mr. Jeffery.

On

On the 31st of March, 1810, certificates were received from Mr. John Bice, Cotton-End; Mr. J. Hawkins, Castle-Ashby; and Mr. William Shaw, Hardingstone, to the following effect: viz.—That they had purchased of Mr. William Jeffery, and made perfect trials of his newly-invented expanding harrows, and find them to be upon a much superior principle to any they have seen, or made use of before. Testimonies of their use.

Reference to Mr. W. JEFFERY'S *Expanding Harrows.*

Fig. 3, Pl. VIII, represents Mr. Jeffery's expanding harrow. It consists of two sets of movable bars of wood, connected by hooks in one set, and eyes in the other. Each set is composed of four bars of wood, A B C D, furnished with teeth; these are connected, and held parallel to each other by three other bars, or braces, E F G, united to the former by screw bolts; the iron loops H I are the points for the chains, by which they are drawn; K are two iron braces, joined to the bars E at one of their ends, and have a number of holes, any of which can be put over screw-pins fixed upon the middle bar F, provided with nuts; when these nuts are removed, and the iron braces detached from their pins, the frames may be either closed up, or extended, so as to bring the teeth of the harrow nearer together, or remove them farther asunder, and they can be fastened at any point by the different holes in the iron braces, so as to be worked with the teeth at any requisite degree of extent. The harrow described.

XIII.

Observations on an occasional Increase and Decrease of Bulk in the Hair of the Head. In a Letter from THOMAS FORSTER, *Esq.*

To WM. NICHOLSON, Esq.

SIR,

IT has always appeared to me, that the best means to get at a correct knowledge of any intricate subject is, to excite the Knowledge gained by ex-

citing attention to a subject.

the attention of others toward it; particularly of those who may have more extensive opportunities, as well as more capacity for accurate observation than myself. In conformity with this view of the subject, I request your insertion of the following observations.

Sympathy between the skin and stomach.

The sympathies between the skin and the stomach have been frequently adverted to by physiologists; the skin has been found to be alternately *hot and dry*, *hot and moist*, *cold and dry*, and *cold and moist*; and these varieties have been attributed to variations in the state of the stomach, between which and the skin a very direct sympathy is believed to exist. But the variations in the appearances of the hair do not appear to be duly noticed.

Variations in the appearance of the hair

Causes of apparent increase of quantity.

The body of the hair enlarged.

I have remarked, that people of what is usually called nervous and susceptible constitutions appear at times to have but half the quantity of hair on their heads, that they have at others, though they have assured me none had been cut or combed off. On minute examination I have found, that the apparent increase in quantity at certain times was occasioned by the following circumstances: the shafts themselves were found to be specifically larger, and more tense or elastic, at the same time that they did not lie in such close contact. The apparent diminution in quantity, at other times, I found to result from a specific decrease in the size of the shafts, which also lay in closer contact than ordinary, and were more flaccid, and generally more dry. Considering the considerable influence which the atmosphere exercises on our bodies, I was once induced to attribute the *closer contact* of the shafts to a diminution in their *electricity*, by which they would become less *mutually repulsive*; this however does not seem calculated to account for their increase in size.

What is the cause of this?

May the shaft be considered to be organized throughout, and its enlargement to be caused by an increased action of its vessels? or, Is there an aëriform perspiration into the cavity of the shaft, on an increase of which it becomes distended? or may the increased tension and size of the shaft be considered as resulting from the cooperation of these two causes?

Apparently connected

The strength and tension of the hair appears generally to accompany health, while the weakness, close contact, and flaccidity

flaccidity of it denote disorder. I have observed also, that small doses of mercury have changed the appearance of the hair very soon after their administration. From being flaccid, dry, and small, it has become tense, strong, and moister; at the same time more tension and solidity has appeared in the muscles, and the countenance has displayed a more healthy appearance. Now mercury may increase an aëriform perspiration, (if such a one exist) into the shaft; it may also set the digestive organs to rights, thereby cause a more healthy action of the vessels in general, and of those of the shaft among the rest. I cannot help observing, that there is no objection to supposing hairs organized, because we cancannot discover their vessels. On this subject we may, I think, be allowed to reason thus: If all nourishment be performed by the action of vessels, either vascularity must extend itself *ad infinitum*, or there must be certain small vessels not nourished at all. Can we demonstrate those small arteries, which ramify in the coats of and nourish the smallest *vasa vasorum*? Such considerations as these ought to prevent our denying organization to any part of an animal body, even to the cuticle and the enamel of the teeth.

with health and disease. Mercury soon restores the healthy appearance.

Organization probably extends much farther than is generally supposed.

I shall be much obliged to any of your correspondents, who may have noticed any connection between the varieties in the appearance of the hair and any peculiarities in the state of the body, &c., to communicate them in your scientific journal; and I remain, Sir,

Your constant reader,

THOMAS FORSTER.

XIV.

On the Prevention of Damage by Lightning. In a Letter from Mr. B. COOK.

To Mr. NICHOLSON.

MY DEAR SIR,

I HAVE read with much concern almost every week for some time past accounts of some damage of one kind or other done to buildings, trees, and cattle, or in the loss of lives by lightning;

Annual damage done in this country by lightning.

lightning; indeed every year this country suffers very much, either by the destruction of trees, houses, and cattle, and what is far more distressing, the loss of so many lives by the electric fluid. I have endeavoured to form an idea of the loss sustained on an average; and I find upon a moderate calculation, it cannot be far short per annum of 40 to 50 thousand pounds, and the loss of lives from 20 to 30. It is of so serious a nature, that I wonder no effort has been made to remove, if not wholly, at least a part of the evil. Looking at it in this light, and conceiving it to be the duty of every man to endeavour to propose some remedy, I have taken the liberty to hand you what follows for your consideration; if you think it worth inserting in your valuable Journal.

Our kingdom from its high and rocky nature, from its bowels containing such vast masses of iron, copper, and other ores, all conductors of lightning, and also from its situation in the midst of the waves, itself becomes a conductor also; all these circumstances conspire to collect the electric fluid together around us. If it was possible to find out means to carry off this very destructive element without danger, the country would experience a great and invaluable benefit from it. The loss of so many lives is a very serious consideration, and ought to engage the studies of the philosopher and philanthropist to propose some remedy, if only for their sakes, and if it is impossible to remove the evil wholly, at least it is possible to remove it partially.

Plan proposed for preventing it.

The plan I with much deference propose, and I feel satisfaction in proposing it first to you for your consideration, because if you do not approve of it, it will not meet the eye of the world, for no man is more competent to decide upon its merits than yourself. The plan is to erect at different stations conductors throughout the kingdom, at 5 or 6 miles distant, or in some instance nearer, according to the nature of the ground, on the most elevated parts, so that wherever the clouds moved, surcharged with the electric fluid, the conductors would carry it down, so that it would be next to an impossibility for a collection of electric fire to accumulate, so as to produce a destructive discharge. I have very little

little doubt, but that all, or nearly all of the fluid would be carried off by these conductors, and little or no damage, or death would ever be occasioned by the lightning.

The expense of erecting conductors at different stations throughout the kingdom would be saved in a few years, and the safety of men's lives would be of more value than any expense that could be incurred. If every parish would agree throughout the kingdom to appropriate a part of the rate for the erection of 4, 6, or more conductors, according to the size of the parish, on the different parts that are most elevated, the expense would not be felt—indeed it would not be worth naming. If the different noblemen, gentlemen, &c., of the different parishes were to take it into consideration, first considering the certain security it would provide for their cattle, buildings, and the lives of themselves and servants; and secondly, when they estimate the very small expense these conductors might be erected for; I do think every parish would instantly be induced to adopt the plan.

Faults of the common iron conductors.

But there are several great imperfections and objections against the present iron conductors.

The first is, the very short time they stand without being deeply corroded with rust, and when first put up the iron is so very irregular on the surface, that it is a great hindrance to the descent of the electric fluid, and calculated in a great measure to cause it to fly off to any other conducting substance in its way or near to it; and when up for a few years it becomes still worse, and so incrusted with rust, that the irregularity and imperfections of the conductor are increased. Another fault is, that the tops of the conductors are not raised high enough above the building they are placed to protect; the point of the rod is in general placed just above the chimney. The rod ought to rise 6 or 8 feet above the top of the house or building, and to end in a single point only. If conductors are used, in every instance the best materials should be used to make them. Iron is the very worst material, and yet all conductors are made of iron; but this arises from the cheapness of the article.

Conducting power of different metals.

According to the experiments of Mr. Henly, (published in Dr. Rees's Cyclopedia, under the article Conductors),

to prove the best conductors, he found the same charge from an electric machine melted 4 inches of gold wire, 6 inches of brass wire, 8 inches of silvered copper, 10 inches of silver, and 10 inches of iron *wire*; so that gold is the best conductor, and iron the worst. Brass stands next to gold in the quality of a conductor. Cavallo says, that copper and brass are the best conductors, and also that they never rust; but to make them of copper or brass would be a very great expense, and then, if not drawn through plates, they would be very uneven on the surface, which is a defect in electric rods.

Iron tubes plated with brass.

I had in prospect the making of conductors on an improved plan, so that they would be equal to solid brass in their use, and come as cheap or cheaper than wrought iron, in a patent I have very recently obtained for combining different sorts of metals, particularly brass or copper, with iron. By this method we can plate or cover tubes of iron 15 or 16 feet long, of any diameter, with a coat of brass, from $\frac{1}{16}$ of an inch, to any thickness; and so connected with the iron, by compression, that, when so combined, it appears a solid piece of brass, but being hollow, is very light and portable, and the method used in making them being by drawing them through a polished draw plate, all the surfaces are as smooth and uniform as it is possible to make them. Being made in convenient lengths, they may be sent to any part of the kingdom, and put up in a very short time, as one piece screws into another, so that, when screwed in, both edges of the brass meet, and join together. Conductors of this kind would never rust, as what is presented to the atmosphere is brass only. They would be $\frac{1}{4}$ lighter than iron rods, would be put up in a very short time, would be quite as cheap as iron, and furthermore would be the best conductors you can possibly make. But as I said before that I had given this subject a good deal of thought, especially the probability of drawing off the electric matter by conductors, so as to prevent its getting to a head and causing by its discharge so many accidents; when I considered the manner of the iron rods and their great defects, it set me a thinking how I could contrive a better conductor than iron, and I flatter myself I have succeeded. Therefore I leave it to every man to judge, whether

whether what I have asserted is true, namely, the great damage done yearly by lightning, and also the great necessity of providing, if it is possible, some remedy; and if conductors are the only means that promise a remedy, those conductors which afford the most beneficial and lasting results will certainly be chosen. *This is, Sir, very much like recommending my own invention*: but if my rods are the best, which I leave to every candid man to judge; and if society is benefitted, I see no reason why I should not be benefitted also. The present conductors on shipboard, where any are used, are I believe constructed of chains, which are the worst of all conductors, as the lightning has to run down the most irregular of surfaces, besides their being so clumsy. But my brass rods might be so attached to the mainmast, and the collecting point raised above the top; and where the joints of the mast are, there might be a round universal joint, that would bend in every direction with the mast. The rod might be carried down thus into the sea, and the expense of them would be so trifling, that one would hardly think any vessel would be without one, especially when it is considered, they would be made of a metal allowed by all who have written on electricity to be the best conductor of lightning. Conductors for ships.

I am, Dear Sir,

B. COOK.

Annotation. W. N.

The subject of conductors for lightning being still obscure, I have with pleasure inserted Mr. Cook's communication without considering, as at all needful, that an acquiescence in its contents should be implied throughout on my part. Being founded on the generally admitted doctrine, it is in many respects entitled to consideration, and, like all other ingenious researches, is calculated to excite investigation. On the present occasion I would remark, that the course, disposition, and striking places of thunder clouds appear to be governed in a very great measure by Conductors for lightning.

certain

Conductors for lightning.

certain conducting parts lying along or within the earth either as ridges or internal masses, and that the stroke from a stratum of clouds, many miles in length, seems to be determined by an action which extends far beyond the influence of any metallic rod,—even supposing this last to be inserted into the conducting mass itself: that the whole process of atmospheric evaporation and condensation appear to be accompanied with electric phenomena upon a very extended scale, but most strikingly manifest when the changes are rapid; this last being the only difference between thunder storms and common squalls or showers: and that it does not seem probable, that our rods can essentially modify the course of these effects. Other more remote considerations would offer, if they could; such as the possibility of an interruption of the ordinary course and frequency of showers, which Darwin thought within the reach of human power, and the greater probability that the atmospheric electricity of a whole country would soon destroy any series of conductors: but the affair of the poor-house at Heckingham *, in Norfolk, which, about thirty years ago, was struck and set on fire by lightning without touching any one of eight elevated metallic conductors attached to the building, has been considered as a proof of the very limited influence of these rods, and that their power of protecting a single edifice requires the condition, that all the conductors should be connected together, and with the metallic parts of the house.

XV.

Extract of a Letter from Mr. CORDIER, *Mine Engineer, on Mount Mezin* †.

Mount Cenis. Alternation of gypsum and micaceous schist.

THE passage of Mount Cenis has been laid open to view by the new road. We see there vast strata of gypsum, which alternate with the rocks of micaceous schist, compose nearly a twentieth of the mass of the mountains, and show them-

* See Philos. Trans. of that time.

† Journal des Mines, Vol. XXVI, p. 233.

selves

selves equally in the lowest and in the highest parts of these mountains. Saussure had supposed this gypsum to be superposed, but I easily satisfied myself, that it is in reality intercalated.

I have revisited almost all the extinct volcanoes in the interior of France. My object was to verify many of my descriptions, and to make new ones, wherever I could find situations truly classical, that is, capable of being cited as exhibiting a complete and perfectly circumscribed geological phenomenon. Extinct volcanoes in France.

I have paid much attention to Mezin, which is a volcanic system analogous to Puy-de-Dôme and Mont-d'Or, but much better characterized. We see there two orders of volcanic substances; those that were anterior to the last period of deluge, and those that have been thrown out subsequent to all the revolutions the Earth has undergone. The mass of the mountains is composed almost wholly of primitive formations. Considered as a whole, it is a frustum of a very obtuse cone of ten leagues radius. I find, with Mr. Ramond, that it is 1774 met. [1939 yds] above the level of the sea, and about 800 met. [874 yds] above the granitic flat on which it rests. It exhibits the ruins of a volcanic colossus, unquestionably much loftier and more extensive. We find in it this very remarkable peculiarity, that most of the incoherent matters thrown out have undergone no alteration, and have not been changed either into tufs or breccias. The red scoriæ in fragments, and the black stony scoriæ, appear with all the characters impressed on them by the fire. Add to this, all the currents, or segments of currents, are accompanied with scorified crusts above and below. The interior of these currents presents only lithoid lavas, from the basaltic porphyry to the compact earthy, or fine-grained granular porphyry with base of feldspar. The three varieties with base of feldspar are frequently found in the same current, and thus exhibit the transition of the three pretended species, domit, the base of graystone, and clinkstone.

Mount Mezin. Two orders of volcanic substances. The mountain described. The volcanic substances unchanged. Transition.

The modern lavas are not very numerous at Mezin. They are all formed of porphyritic basaltes with fine crystals of peridot, and pyroxene, mixed with nodules of granular peridot. The later lavas.

peridot. The same nodules and the same crystals are found in the scoriæ that compose the craters, whence these lavas issued. The modern currents having almost all flowed through narrow and deep valleys, the torrents have resumed their beds, by hollowing out vast furrows in the lava. Hence result sections admirable for their height, which sometimes reaches to 200 French feet; for the regularity and dimensions of the basaltic columns; or for their extent, as they frequently reach whole leagues. These superb curtains are ornamented with scoriæ at top and bottom. The decomposition of the lower scoriæ gives rise in certain places to a curious phenomenon. The tuf, or wacke, resulting from it, mixes with the river-mud or sand, which the lava had covered, and these places exhibit a transition of the sort that Werner admits: that of sand, or clay, to basaltes! The modern basaltic columns of Mezin are unquestionably the finest ever yet observed.

Transition.

The whole system of Mezin rests on a new kind of granite, into which pinit enters in the proportion of a twentieth, a tenth, and even a third. This rock occupies a space of more than 250 square leagues, and extends to what was formerly Forêt, where it serves as a matrix to the substance that was taken for emerald, but is only a translucid pinit. Of this I satisfied myself on the spot.

New kind of granite.

SCIENTIFIC NEWS.

Report of the Proceedings of the Mathematical and Physical Class of the French Institute.

(Concluded from p. 240.)

Respiration of fishes.

SINCE Mr. von Humboldt's return to France, he has made many experiments on the respiration of fishes, in concert with Mr. Provençal. Spallanzani and Sylvestre had shown, that fishes do not breathe by decomposing water, as some had supposed, but by water obstructing the oxigen dissolved in it, or by coming to the surface to collect oxigen

gen directly from the atmosphere. The experiments of Messrs. von H. and P. have gone farther. Seven tenches were placed under a jar filled with river water, containing 4000 cent. cub. [243·6 cub. inch.]. After living in it eight hours and a half, the analysis of the air still found in the water showed, that the fishes had absorbed in this time 145·4 [8·85 cub. in.] of oxigen, and 57·6 [3·5] of nitrogen, and that 132 [8] of carbonic acid had been produced.

Effects of different gasses on them.

In water deprived of air the fishes were uneasy, and in about twenty minutes fell motionless to the bottom. In pure oxigen they appeared to respire eagerly, and spread their gills more. In nitrogen and hidrogen they kept their gills closed, seemed to dread the contact of these gasses, and died soon after they were put into the water containing them. Carbonic acid too kills them in a few minutes. But it is not by their gills alone that fishes absorb oxigen and nitrogen, the whole surface of their bodies has the faculty of acting on these gasses. After the fishes were taken out of the water containing the deleterious gasses, a small portion of carbonic acid was found in it, exhaled probably from their bodies.

Respiration of animals after the 8th pair of nerves was divided.

Mr. Provençal has also made some experiments on the respiration of mammalia after the eighth pair of nerves had been divided. The animals gradually absorbed less oxigen, and produced less carbonic acid, after the operation. At first their respiration was not apparently weakened; but it soon became feeble; and at length ceased altogether: probably from the cessation of the mechanical functions of the thorax. The heat of the animal diminished soon after the division of the nerves, and proportionably with the respiration.

Airbladder of fishes.

With the functions of the airbladder of fishes we are not yet well acquainted. In some it has a duct communicating with the stomach. In others this duct is wanting, and it contains a peculiar organ of a red colour, and a laminated structure. In some both this organ and the duct are found, and in a few this bladder has muscles. The air contained in this bladder is a mixture of oxigen and nitrogen, the former being in greater quantity in proportion to the depth at which the fish lives in the water. Its absence does not appear

appear injurious to respiration, though it does to the production of carbonic acid. Tenches from which it has been removed swim, dive, and rise to the surface, with as much ease as others. Such are the principal results of the different inquiries of Messrs. Duvenoy, Delaroche, von Humboldt, Provençal, and Cuvier.

Poison of the upas.

Drs. Magendie and Delisle have made many experiments on animals, chiefly dogs, with the poison of the upas. Whether introduced into the system by the bloodvessels or lymphatics, by the way of the intestines, or by a wound, the animals died universally convulsed. It appears particularly to affect the spinal marrow, and to enter the system only by means of the circulation. It seems to act but very indirectly on the brain, thus showing an independence between it and the spinal marrow, that is not indicated by anatomy.

Juice of deadly nightshade.

Mr. Vauquelin has found, that the juice of belladonna, when swallowed by animals, produced in them a delirium exactly similar to that of opium. Some experiments of Mr. Sage confirm the action of this juice on the nervous system.

Gasses injected into the bloodvessels.

Dr. Nysten has examined the effect of different gasses injected into the bloodvessels. Atmospheric air, oxigen, nitrous oxide, carbonic acid, carbonic oxide, sulphuretted hidrogen, &c., were not deleterious. Oximuriatic, ammoniacal and nitrous acid gasses appeared to act by irritating very violently the right auricle and pulmonary ventricle. Sulphuretted hidrogen, nitric oxide, and nitrogen were injurious to the contractibility of these parts. Some others so changed the nature of the blood, that respiration was unable to convert it from venous to arterial.

Stings of insects and fishes.

In a paper on the means of remedying the sting of the weever, trachinus draco L.; and on the effects of the poison of the tarantula, with the mode of cure used in Spain; Mr. Sage recommends the internal and external use of the volatile alkali.

Rotations of crops.

In a report on the Means of improving Agriculture by Rotations of green crops by Mr. Yvart, the committee recommends it as answering the important purpose of showing how land may be rendered constantly productive without exhausting it.

Mr.

Mr. de Cubière read a paper on the cultivation of the bald cypress (*le cyprès-chauve*), showing the advantages of this fine tree. Culture of the cypress.

Mr. Leblanc, who has resided several years in America, communicated his ideas of the ease with which the vicugna might be domesticated in the Alps and Pyrenees. Vicugna.

Mr. Poyfère-de-Ceré read an account of the mode of washing superfine wool in Spain. Wool.

Mr. Percy related some curious observations on the fabrications of the jars and alcarazas, which the Spaniards employ for preserving liquors, and for cooling them. Alcarazas.

In the report of the Class of History and ancient Literature, a paper by Mr. Grégoire is mentioned, containing a description of a singular ancient bell, from the convent of Bobbis, in Piedmont. This bell is about 9 dec. [35·4 inches] in diameter, and of a spherical shape: one hemisphere being complete; the other formed of ten branches, broad at the base where they join the upper half, and tapering to a point*. Its sound is much louder than that of a bell of the common form of the same weight. A small portion, taken from the ear, was analysed by Mr. Vauquelin, and found to consist of 76 parts copper, 20 tin, and 4 lead. Mr. Vauquelin was satisfied, that these were the only metals present; though, from the smallness of the quantity analysed, the proportions may not be strictly accurate. He supposes, that the lead was an adulteration of the tin, though advantageous to the sound. Messrs. Molard and Montgolfier have cast four other bells of the same form and size, before they had a knowledge of Mr. Vauquelin's analysis, using different compositions. That which came nearest in sound to the original was a mixture of equal parts of copper, brass, and tin. Ancient bell of very loud tone.

Royal Society of Sciences at Harlem.

The prize for the question concerning the insects most injurious to fruit trees, their natural history, and the means Insects injurious to fruit trees.

* Nothing is said of the thickness of the metal, or of the space left between the points of the branches, which appear from the description not to be united. C.

of

of destroying them, was awarded to Mr. Fred. W. Freyer, councellor of the court and of the regency of Saxe Hilburghausen.

Prize questions.

The following questions, having received no satisfactory answer, are repeated,

Graduation houses for making salt.

1. May graduation houses, for making salt from seawater be established with advantage on the coast of Holland; and how may they be best conducted, considering the circumstance of the country?

Effects of manures.

2. From the process lately made in the physiology of plants how far do we know in what way vegetation is promoted by different manures in various soils; and what indications may we deduce from the knowledge we have acquired, with respect to the fertilization of uncultivated and barren land?

Ancient topography of Holland.

3. How far can the study of ancient authors, the examination of antiquities, and observations made on the spot, serve to determine with certainty what the face of this country was formerly, particularly under the dominion of the Romans, including the course of the rivers, and extent of the lakes, and what changes they have successively undergone?

Changes on the coasts of Holland.

4. What do historical accounts of acknowledged authenthenticity teach us of the changes, that have taken place on the coasts of Holland, the islands, and the arms of the sea that separate them? and what useful information may be derived from it.

Ancient and present height of tides.

5. Do the tides on our coasts rise higher than in former ages, and fall proportionably less low?—If so, how far can we determine the quantity of this difference in ages more or less remote, and what are the causes of the changes? Do they arise from gradual alterations in the outlets of the waters, or do they depend on external and more remote causes?

Renovation of the oxigen of the atmosphere.

6. As the experiments and observations of philosophers have shown of late, that the quantity of oxigen gas emitted by plants is by no means sufficient to supply to the atmosphere what is consumed by the respiration of animals, combustion, absorption, &c., by what other means is the due proportion between the component parts of the atmosphere continually preserved?

7. How

7. How far has chemistry made known the component parts and principles, both proximate and remote, of plants, particularly of those employed as food? and how far can we deduce from what is known, or what may be discovered by experiments, combined with the physiology of the human frame, what vegetables are best adapted to our use in a state of health, and in certain diseases. Immediate and remote principles of plants.

8. What is the cause of the phosphorescence of the water in the seas and inlets in and around this kingdom? Does the phenomenon depend on the presence of living animalcules? If so, what are they, and are they capable of imparting to the atmosphere any injurious properties?—They who purpose to answer this question are requested to consult the most recent and accurate writers on this subject, particularly Viviani, Genoa, 1805, and to examine how far this phosphorescence, which is very remarkable on some parts of our coasts, is connected with the prevailing diseases in unhealthy seasons. Phosphorescence of the sea.

The following new questions are also proposed.

9. As the secretion of milk in cows appears to be increased, when they are fed in stables on potatoes, carrots, or beetroots, it is required to show by experiments and observations, whether the milk of cows be really increased by these articles of food, and under what circumstances; in what way they can be given with most advantage; whether the quality of the milk be altered by this feeding; and, if so, what this alteration is, particularly with regard to the quality and quantity of cream and butter produced. Milk of stall fed cows.

10. As the antiseptic quality of common salt appears not to depend solely on the muriate of soda, but also on the muriate of magnesia, which adheres to common salt, it is required to determine by experiments the comparative proportions of the antiseptic quality of these two salts; in what proportions they should be mixed, to prevent putrefaction as long as possible, without the taste of the substances we would preserve becoming less agreeable; and whether it would be advantageous to use muriate of magnesia alone, particularly in voyages to hot climates. Antiseptic quality of the muriates of soda and magnesia.

11. What

Shell lime.

11. What is the chemical reason why stone lime makes on the whole more firm and durable buildings than shell lime, and how may the latter be improved in this respect?

Nitre beds.

12. May nitre beds be profitably formed in this country, particularly in places where the water is impregnated with several substances produced by the putrefaction of animal matters? and what rules should be observed respecting them?

Nature of luminous meteors.

13. What do we know, from incontestible observations, of the nature of luminous meteors, or those that have the appearance of fire, lightning excepted, which occasionally appear in our atmosphere? how far can they be explained by known experiments? and how much is there still gratuitous or doubtful in what the philosophers of the present day have asserted respecting them?

Metals from the alkalis.

14. Can it be demonstrated by incontrovertible experiments, that the substances which have the appearance of metals, produced from alkaline salts, are real metals? or are there sufficient reasons to maintain, that they are hydrurets, produced by the combination of hidrogen with the alkalis? What is the most certain and convenient mode of producing these substances from the alkaline salts in pretty considerable quantity by means of a high temperature?

An omnia ab ovo?

14. How far may we still maintain the doctrine of Harvey, that animals are born in general from preexisting eggs, and that plants spring only from seeds? and on the contrary what are the principal observations that show, that there are animals and plants, which are produced in a different mode?

Chemical explanations of electricity.

16. What judgment is to be formed of the chemical explanations attempted to be given of electrical phenomena? Are there any founded on sufficient experiments, or that may be proved by new ones? Or are they to be considered hitherto as hypotheses by no means proved, or advanced without valid reasons?

The time for answering all these questions is previous to the 1st of January, 1812. Beside the usual medal, value 30 duc. [£13 : 17 : 6], 30 ducats in addition will be given to those who answer questions 2, 3, 4, 7, 10, 11, 13, 14, and 15; and 50 duc. [£23 : 2 : 6] in addition to questions 1 and 5.

Academical

Academical Society of Sciences, at Paris.

At the meeting in September, 1809, Mr. Nauche communicated some experiments he had made on the contraction of the muscles in frogs. The object of these was to show, in opposition to the assertions of prof. Richerand and Bichat, that the contraction of the muscles may take place independently of nervous influence, or the influx of the blood. We learn from comparative anatomy, that this is the fact in thousands of animals, which have neither blood-vessels nor a nervous system, yet this faculty is excited in them with great intensity. It may even be affirmed, that it is more active, stable, and independent of life, in proportion as the animal is less perfect, and lower in the zoological scale. These facts appear to confirm the opinions advanced by Mr. Dubuisson in his Essay on the Properties of the Vital Power in Vegetables, that contractibility, or irritability, is inherent in the muscular fibre, and peculiar to it; as sensibility is to the medullary substance, renitence and elasticity to the albugineous fibre, and tone to the cellular texture.

Contraction of the muscles independent of nervous influence and the blood.

Society of Encouragement at Paris.

A report on the thread stockings manufactured by Mr. Detrey, sen. says, that they combine fineness, strength, and beauty. The evenness of the thread, its lustre, and accurate spinning are remarkable. They are three thread. They are not dear, as the price is 15 fr. [12*s.* 6*d.*] a pair; and when it is considered, that cotton stockings are manufactured in France as high as 48 fr. [£2.], not exceeding them in beauty, and inferior in strength, they must be esteemed a public benefit.

Thread stockings.

The gold and silver medals, on count Rumford's donation, have been adjudged by the president and council of the Royal Society to Mr. Malus, for his discoveries of certain new properties of reflected light, published in the 2d vol. of Mémoires d'Arcueil, which we shall insert the earliest opportunity.

Count Rumford's medals.

The

Jacksonian premium.

The Royal College of Surgeons in London have awarded the Jacksonian premium of £10, and an extraordinary premium of £10, to Mr. John Smith Soden, of Coventry, and to Mr. James Gillman, of Highgate, both members of that college, for their dissertations on the *Bite of a Rabid Animal*, from the consideration, that such two dissertations are highly meretorious productions, and are equally worthy of the Jacksonian prize.

Cotton manufactory in Italy.

The cultivation of cotton, and its manufacture, are said to be carrying on to a considerable extent in Italy.

Vessel of a new construction.

Mr. Daubusson de la Feuillade has exhibited on a canal near Paris a model of a vessel of his invention. The model was 25 feet long, 52 inches broad, and did not draw 5 inches of water. The inventor proposes to build one 200 feet long, and 50 feet beam*, which is to carry 66 guns. It is to have four masts, be deep waisted, and with two thousand men, and provision for 50 days, would draw only 9½ feet of water. It will sail faster, and lie nearer the wind, than any other vessel. Its sails are to turn quite round, and either end may be made the stem or stern at pleasure, so as to render tacking unnecessary†. In a dead calm a *rame aspirante* is to supply the want of wind. Cork sheathing, and airvessels of copper, are to be employed, to render it incapable of being sunk. Its intention is to surprise an enemy's harbour.

Valuable writing ink,

An inkmaker at Paris professes to have discovered a vegetable fluid ink, which never thickens or loses in any degree its fluidity by evaporation, is always free from sediment, and never occasions iron moulds, or in any way injures linen or clothes, that may be accidentally soiled with it. He adds, that what is written with it never becomes yellow by age. Sonnini says, that he has long used this new ink made by his neighbour, Mr. Alphonsus Wée, and that it possesses the qualities ascribed to it.

* This is not proportional to the model. C.

† The inventor appears from this to have but little knowledge of navigating a ship. C.

A
JOURNAL
OF
NATURAL PHILOSOPHY, CHEMISTRY,
AND
THE ARTS.

SUPPLEMENT TO VOL. XXIX.

ARTICLE I.

Method of assisting the Escape of Persons, and the Removal of Property from Houses on Fire: by Mr. JOHN DAVIS, *No. 7, John Street, Spitalfields* *.

SIR,

I BEG you will have the goodness to lay before the Society of Arts, &c. a machine, which I have invented, for more effectually saving persons and property from fire. Machine for saving persons and property from fire.

It appearing to me a desirable object, that the public should be in possession of an apparatus better adapted for the above purpose than any now in use, I have endeavoured to strike out an entire new plan of a machine calculated for the use of a parish, which can be easily removed and adjusted to any window, with a convenient apparatus or box, movable up or down, so as to receive persons or property. I have completed such a machine, which has answered my expectations, and been approved by several gentlemen who have seen it in action.

* Trans. of the Soc. of Arts, vol. xxviii, p. 175. Fifty guineas were voted to Mr. Davis for this invention.

The machine is at Mr. James Bevan's mahogany-yard, City road, where it shall be exhibited to a committee appointed by the society whenever they please.

I am, Sir,

Your obedient humble servant,

JOHN DAVIS.

No 7, *John Street*, *Spitalfields*,
Jan. 10, 1809.

Reference to the Engraving of Mr. John Davis's Fire-Escape, *Pl.* IX.

Description of the machine.

The plan of my fire-escape is calculated for the use of a parish; its principle consists in three ladders, ABC, applied to each other by four clasp irons on the top of each of the two lowermost, which are so contrived that each ladder may slide into the one beneath it; on the top of the lowermost ladder A two pullies are fixed on the inside, over which two ropes *a a* pass, and situate between the lower ladder A, and the middle one B. The ropes are made fast to the bottom of the middle one on each side in a proper direction with the pullies on the top. The upper ladder C is attached to the middle one in the same manner, and on the top it carries two horn pieces, D, made of iron, and turned off at each end similar to two horns, which are four feet wide; their ends are sharp to pitch on each side of a window, and with its points hold the ladders steady. The three ladders when shut down are about fifteen feet in height. They are placed perpendicularly in the middle of a framed carriage, EF, of nine feet six inches long, and five feet six inches wide, mounted upon four wheels F. On each side of the carriage a windlass is placed; that marked G on the right side of the carriage is for the four ropes *a a* and *b b*, fixed two to each ladder AB. By turning this windlass the ladders may be wound out from their standing height of fifteen feet to forty. Over this windlass is a screw turned by the winch *d*, by turning which the ladders may be inclined against the house with all imaginable ease. On the top of the upper ladder C on the outside, are two pullies, over which two chains are conducted to the windlass H on the

the left side for the purpose of carrying up a box I; two of which travel with the fire-escape, so that in the event of one being filled with small valuables, it may be unhooked, and the other K put on, which will save time. The whole apparatus may be drawn by one horse, or six men, and when arrived at the scene of danger may be adjusted in two minutes. If every parish would provide one of these escapes, and keep it where it might be brought out on the first alarm, I feel persuaded it would lessen the many accidents, which occur by fire in the metropolis.

I have the honour to be, Sir,

Your obedient humble servant,

JOHN DAVIS.

No. 7, John Street, Spitalfields,
May 14, 1810.

Farther Observations from Mr. Davis on his Fire-Escape.

SIR,

I BEG leave to return you, and the gentlemen of the committee, my sincere thanks, for the kind attention I have experienced; and should you think the following hints likely to give any additional information on the subject of my fire-escape, you will have the goodness to submit them to the consideration of the committee.

Hints with regard to escape from fires.

Certain it is, that, however good any principle may be, the practice must also be so to be effectual; therefore it is my opinion, that every parish should be provided with a machine on my principle, to be kept in some convenient place, easy of access. The key should be kept at the watch-house by night, and by day at the nearest public-house; if this, which ought to be, were the uniform custom, it would soon become familiar, and be attended with no expense. On the alarm of fire, I would have the machine brought out directly, as I consider it an improvident method, when a house has been on fire some time, and some unfortunate sufferer should appear in need of prompt assistance, to have to search about for the keys of a churchyard, or some other obscure place to bring the fire-ladders; which, when brought,

 if

if not exactly the right height, are useless; and when this, which is not unfrequent, is the case, the remedy is almost as bad as the disease, witness Mrs. Smith, having fallen off a parish ladder, at Chelmsford, while endeavouring to save herself in that dreadful fire, in March, 1808. It would be needless for me to enumerate instances, where a well-timed outward apparatus would have been of essential service—the thing is self-evident, and the occasions for their use have also been many. I would also propose, that a board should be put up, offering a reward sufficient to stimulate persons to bring the machine—for example, ten pounds for every life saved by it. I think no person would think it too much, who had been saved. This would have the good effect of having it always in time, which is most essential, as twenty shillings are not sufficient to induce men to the necessary trouble attending such labour.

Having thus offered my sentiments, respecting the good effects which may be derived were certain regulations put in force,

I remain, with great feeling for suffering humanity,

Sir,

Your most obedient and humble servant,

JOHN DAVIS.

II.

New Method of applying the Filtering Stone for purifying Water: by Mr. WILLIAM MOULT, *No. 37, Bedford Sqaare* *.

SIR,

Inconveniences in the common mode of using filtering stones.

IF you think the following information, relative to a new method of filtering water, is deserving of the attention of the Society of Arts &c., I wish you would lay it before them.

* Trans. of the Soc. of Arts, vol. xxviii, p. 212. The silver medal was voted to Mr. Moult.

My

My objections to the old method of filtering by putting water into the filtering stone are, that the dirt falls to the bottom, and fills up, or chokes the pores of the filtering-stone, so that the stone requires frequently to be cleaned with a brush and sponge to allow the water to pass, after which the water passes through the stone in a muddy state for two or three days; it likewise requires to be frequently filled, and as it empties less water comes into contact with the stone, and therefore a smaller quantity, in such a state, can only pass through. Likewise a filtering stone used in the common way soon becomes useless, from the filth insinuating itself into the internal parts of the stone, out of the reach of the brush.

In the method I propose and practise, the filtering-stone is placed within the water to be purified, which presses upon the outside of the filter, and the stone does not require to be supported in a frame as it needs only to stand within the water cistern; it will thus filter, in an equal time, double the quantity of water procured in the common mode; it fills itself, and requires no cleaning, I have upon this plan used one for more than three years with great success. Improved method.

I am, Sir,

Your humble servant,

WILLIAM MOULT,

No. 37, *Bedford Square,*
April 18, 1810.

CERTIFICATES.

We, the undersigned, having inspected and examined a new mode of employing the ordinary filtering-stone, discovered by William Moult, are of opinion that its superiority over the customary method is so great as to entitle it to particular notice. Testimonies of its advantages.

That it not only supplies an infinitely greater quantity of purified and limpid water, but is capable of preserving its porosity free and pervious for years together, by an occasional self-operation.

That by this valuable process the principal objections to drip-stones is removed, viz. the constant labour they require

to

to keep them clean by means of brushes, without eventually producing the intended effect, and without preventing their being finally rendered useless.

D'Arcy Preston, captain in the Royal Navy;
Charles Gower, M. D.;
Thomas Pitt, Esq. V. P. Wimpole street;
Richard Davenport, Esq. Wimpole street.

Reference to the Drawing of Mr. Moult's Filtering Apparatus, Fig. 1, Pl. X.

Description. AA is the cistern containing the water to be filtered; the filtering stone B is suspended in the cistern by a ring around the inside of it, which catches the projecting part of the stone; the water in the cistern filters through into the stone. D is a siphon, which conveys the filtered water from the inside of the stone into a cistern E, which is the reservoir for clean water. *d* a cock to draw it off as it is wanted. By this mode of filtration the impurities of the water are deposited in the bottom of the cistern A, instead of being left in the bottom of the stone as in the usual mode.

III.

Method of raising a loaded Cart, when the Horse in the Shafts has fallen: by Mr. BENJAMIN SMITH, *No.* 11, *Turnham place, Curtain road, Shoreditch* *.

SIR,

I HAVE taken the liberty of sending you a model, with a brief explanation of the utility of my invention, in order that it may be laid before the Society instituted for the Encouragement of Arts &c., to whose comprehensive judgment and abilities I with great deference submit it for their determination, whether they think it likely to be attended

* Trans. of Soc. of Arts, vol. xxviii, p. 215. Fifteen guineas were voted to Mr. Smith.

with

with the success and utility which I flatter myself it deserves. From the simplicity of the construction and the trivial expense attending it, I presume there will be no bar to its universal adoption. I respectfully submit it to the discernment and decision of the society, who will, I am convinced, give it all the merit and approbation it may deserve.

The reason which prompted me to undertake this business is from having seen a horse, which had fallen down under the immense weight of a heavy loaded cart, where it lay for a considerable time in that painful and dangerous situation, which naturally excited compassion even in the most obdurate heart. Every person frequenting the streets of this metropolis must have witnessed similar scenes; and indeed it surprises me, that long before now some expedients have not been publicly suggested to remove the mischief arising from such occurrences, considering the great encouragement that is given in this enlightened age to all useful improvements.

Horses falling in the shafts of loaded carts.

Having conversed on this subject with persons who possess considerable knowledge of horses, and who constantly employ these noble animals, I find, that horses remaining so long as they usually do in such improper positions, and from being often dragged a considerable distance by fruitless endeavours to raise them, are much endangered in their health and lives, and that their situation upon the stones is more prejudicial than the injury received by the fall.

Much injured.

I flatter myself, that my method will be found to raise the whole weight of the cart, and a considerable part of that of the horse, in the short space of three or four minutes from the moment of the accident, by means simple and useful, and within the reach of the meanest capacity to execute; and that the whole apparatus will not cost above fifty shillings, and will last many years. Requesting your kind attention,

Method of relieving them.

I am, Sir,

Your most obedient servant,

BENJAMIN SMITH.

No. 11, Turnham place, Curtain road, Shoreditch, London, Dec. 13, 1809.

Advantages

Advantages derivable from this Invention:

Advantages. 1.—The invention is of itself so simple, and the operation so conspicuous at the first view, that the whole process may be easily comprehended and executed.

2.—The apparatus may be fitted with little difficulty to any cart now in use for heavy loads, such as bricks, coals, corn, or the like.

3.—The chains, which lead from the uprights at the back part of the cart to the fore part of it on each side, are for the purpose of taking the purchase therefrom, and making the back part of the cart act as a lever at the time the horses are drawing behind, which without fail, with the strength of one, two, or three horses fastened there to raise the one which is down in the shafts, will instantly assist him to get upon his feet.

4.—The number of horses to draw a cart are usually in proportion to the weight contained therein; therefore supposing three horses are employed to draw it, and the shaft horse falls, the carman has only to unhook the two leaders, and then hook them to the short chain at each side of the back of the cart, and with their strength the fallen horse will be so relieved from the weight, as to raise himself without farther assistance.

5.—The same principle may be applied in different ways from what I have shown in the model; for instance, another mode may be adopted by framing the tail-board of the cart strong enough to bear the purchase; and, with the use of the two side chains above mentioned, it may be made to answer the purpose.

Another plan, though more expensive, is by obtaining two wrought iron uprights to be fixed as substitutes for the truss staffs at the back part of the cart, with a hole in the top of each to receive an iron rod, which is occasionally to be introduced, reaching from one side of the cart to the other, connecting the two uprights together; when in action the two side chains to be used as in other cases.

Reference

Reference to the Drawing of Mr. Smith's Method of raising up a Horse when fallen down in the Shafts of a loaded Cart, Fig. 2, Pl. X.

A is the wheel, and B the shafts of a cart, such as is used in London; C the side rails; at the end of the body an iron stancheon or truss staff, *a*, is fixed by the hinge at the lower end, and at the upper end it is supported by a chain *b*, extended from the fore part of the body of the cart; this diagonal chain forms a firm support to the stancheon. This is all the addition made to the common cart, and is used in the event of the shaft horse falling, by hooking the traces of the other horses to a chain *d*, also fixed to the stancheon; the power of these horses, applied at this height above the fulcrum, will have a great purchase to elevate the shafts, and set the fallen horse at liberty, as is evident from an inspection of the figure. The stancheon moves on a joint on its lower end, and the oblique chain unhooks at *b*; the end can be connected with a short piece of chain *e* fastened to the last of the side rails; the stancheon now takes the position of the dotted lines *f*, and the short chain, which hangs down perpendicular from the end of it, may be taken hold of by any number of men, to weigh upon and raise the cart in cases where the horses cannot conveniently be applied; the men will in this manner have much greater effect than merely (as is the common practice) weighing on the hind part of the cart. Explanation of the plate.

When the chain is completely detached, and the stancheon suffered to hang down perpendicularly, it forms a prop to support the cart steady while it is unloaded. It should be observed, that, though only one stancheon appears in the figure, there are in fact two, one being placed on each side of the cart.

CERTIFICATE.—Mr. William Whitehead, jun., of Cadogan place, Sloane street, certified, that he had attended experiments made to ascertain the efficacy of Mr. Smith's invention; that a cart weighing twenty three hundred-weight, loaded with one tun of stones, was raised by means of Mr. Smith's apparatus with ease by one horse. Testimonial.

That he very much approves of Mr. Smith's invention, and

and thinks it likely to be of great service in general practice, more especially on account of the business being effected with little expense. That many carts are already so formed, that very little additional apparatus will be required to complete them for the purpose.

IV.

Method of Ventilating Mines, or Hospitals, by extracting the foul Air from them: by Mr. John Taylor, *of Holwell, near Tavistock* *.

SIR,

I SEND you herewith a drawing and description of a machine of my invention for the ventilation of mines, with a view to their being laid before the Society for the Encouragement of Arts &c., and hope they will meet with their approbation.

I am, Sir,

Your obedient servant,

JOHN TAYLOR.

Holwell, April 9, 1810.

On the Ventilation of Mines, with a Description of a new Machine for that Purpose. See Pl. X, *Fig.* 3.

Importance of ventilating mines.

Next in importance to the means employed for draining underground works from water may be reckoned those, which are intended to afford a supply of pure air, sufficient to enable the workmen to continue their operations with ease and safety to themselves, and to keep up, undiminished, the artifical light upon which they depend. It is well known, indeed, to all who are practically engaged in concerns of this kind, that men are frequently obliged to persevere in their labour, where a candle will scarcely burn, and where not only their own health materially suffers in the end, but

* Trans. Soc. of Arts, vol. xxviii, p. 219. The silver medal was voted to Mr. Taylor.

their

their employers are put to considerable additional expense by the unavoidable hinderance and the waste of candles and other materials.

I mean to confine the following remarks to such mines as are worked upon metalliferous veins, according to the practice of this district, and that of the great seat of mining in the neighbouring county of Cornwall, from which indeed ours is borrowed. We find then, that a single shaft, not communicating by levels to another, can hardly be sunk to any considerable depth, nor can a level (or, as the foreign miners call it, a gallery) be driven horizontally to any great distance without some contrivance being had recourse to for procuring currents of air to make up the deficiency of oxigen, which is so rapidly consumed by respiration and combustion in situations like these, where otherwise the whole remains in nearly a stagnant condition.

We are here unacquainted with the rapid production of those gasses, which occasionally in the collieries are the cause of such dreadful effects; such as hidrogen gas, or the fire-damp, carbonic acid, or the choke-damp; the inconvenience we experience takes place gradually as we recede from the openings to the atmosphere, and seems to arise solely from the causes I have before assigned, though it is found to come on more rapidly in certain situations than in others.

The most obvious remedy, and that which is most frequently resorted to, is the opening a communication either to some other part of the mine, or to the surface itself, and as soon as this is done the ventilation is found to be complete, by the currents which immediately take place, often with considerable force, from the different degrees of temperature in the subterranean and upper atmospheres; and these currents may be observed to change their directions as the temperatures alternate. Usual resource.

The great objection to this mode of curing the evil is the enormous expense, with which it is most commonly attended. In driving a long level, or tunnel, for instance, it may happen to be at a great depth under the surface, and the intervening rock of great hardness; in such a case every shaft which must be sunk upon it for air alone, where not required (as often they might not) to draw up the waste, would cost several Objectionable from its expense.

several hundred pounds; or in sinking a shaft it may be necessary, at an expense not much less, to drive a level to it from some other for this purpose alone.

Attempts to avoid this by a double shaft:

To avoid this, recourse has been had to dividing the shaft or level into two distinct parts, communicating near the part intended to be ventilated, so that a current may be produced in opposite directions on each side the partition; and this, where room is to be spared for it, is often effectual to a certain extent. It is found however to have its limits at no very great distance, and the current at best is but a feeble one, from the nearly equal states of heat in the air on each side. The only scheme beside these, that I know of, has hitherto been to force down a volume of purer air, through a system of pipes placed for the purpose, and a variety of contrivances have been devised for effecting this; most of them are so old that they may be found described in Agricola's work *De Re metallicâ*. The most common are by bellows worked by hand; by boxes or cylinders of various forms placed on the surface with a large opening against the wind, and a smaller one communicating with the air-pipes by a cylinder and piston working in it, which, when driven by a sufficient force, has great power; but the cheapest and most effectual scheme for this purpose, where circumstances will admit of its being applied, is one which I adopted some time since in the tunnel of the Tavistock canal. It is by applying the fall of a stream of water for this purpose, and it has been long known that a blast of considerable strength may be obtained in this manner, which has the advantage of being constant and self-acting. The stream being turned down a perpendicular column of pipes, and dashing in at a vessel so contrived as to let off the water one way, with an opening at another part for the air, which being pressed into it by the falling water, may be conveyed in any direction, and will pass through air-pipes with a strong current, which will be found efficacious in ventilating mines in many instances, as it has likewise, in some cases, been sufficient for urging the intensity of fires for the purposes of the forge. It is easily procured where a sufficient fall is to be had, and the perpendicular column can be so fixed, as that the water from the bottom may pass off, while

or by forcing down air.

Common method of doing this.

Cheaper and more efficacious method.

while the air is forced into a pipe branching from the air-vessel, and which is to be continued to the part of the mine where the supply of fresh air is required.

I have found, however, that the forcing into vitiated air a mixture of that which is purer, even when the best means are used, though a measure which affords relief, is not in bad cases a complete remedy; and where the operation depends on manual labour, or any means that are not unremitted in their action, it becomes quite ineffectual. The foul air, charged with the smoke of gunpowder used in blasting, and which it strongly retains, is certainly meliorated by the mixture of pure air, but is not removed. While the blast continues, some of it is driven into the other parts of the mine; but when the influx of pure air ceases it returns again, or if during the influx of pure air a fresh volume of smoke be produced by explosions which are constantly taking place, it is not until some time afterward that it becomes sufficiently attenuated for the workmen to resume their stations with comfort.

This an imperfect remedy.

A consideration of these circumstances led me to think, that the usual operation of all ventilating engines ought to be reversed, to afford all the advantages that could be desired; that instead of using the machines, which serve as condensers, exhausters should be adopted; and thus, instead of forcing pure air into that in a vitiated state, a complete remedy could only be had by pumping out all that was impure as fast as it became so.

Pumping out the vitiated air preferable.

Many modes of doing this suggested themselves to me, by the alteration of the machines commonly applied, and by producing an ascending stream of air through pipes by a furnace constructed for the purpose. The latter mode would however have been here expensive in fuel, as well as in attendance; and the others required power to overcome the friction of pistons, and so on, or considerable accuracy in construction.

Modes suggested.

I at last erected the machine, of which the annexed is a drawing, which, while it is so simple in construction, and requires so small an expense of power, is so complete in its operation, and its parts are so little liable to be injured by wear, that, as far as I can imagine, nothing more can be desired,

Machine for the purpose described.

desired, where such a one is applied. This engine bears considerable resemblance to Mr. Pepys's gazometer, though this did not occur to me until after it was put to work. It will readily be understood by an inspection of the drawing, Pl. x, fig. 3, where the shaft of the mine is represented at A; and it may here be observed, that the machine may be as well placed at the bottom of the shaft as at the top, and that in either case it is proper to fix it upon a floor, which may prevent the return of the foul air into the mine, after being discharged from the exhauster; this floor may be furnished with a trap-door to be opened occasionally for the passage of buckets through it.

B the air pipe from the mine passing through the bottom of the fixed vessel or cylinder C, which is formed of timber and bound with iron hoops; this is filled with water nearly to the top of the pipe B, on which is fixed a valve opening upwards at D.

E, the air, or exhausting-cylinder, made of cast-iron, open at the bottom and suspended over the air-pipe, immersed some way in the water. It is furnished with a wooden top, in which is an opening fitted with a valve likewise opening upwards at F.

The exhausting-cylinder has its motion up and down given to it by the bob G, connected to any engine by the horizontal rod H, and the weight of the cylinder is balanced, if necessary, by the counterpoise I.

It's mode of action.

The action is obvious.—When the exhausting cylinder is raised, a vacuum would be produced, or rather the water would likewise be raised in it, were it not for the stream of air from the mine rushing through the pipe and valve D. As soon as the cylinder begins to descend, this valve closes and prevents the return of the air which is discharged through the valve F.

The quantity of air exhausted is calculated of course from the area of the bore of the cylinder, and the length of the stroke.

Dimensions of one for large works.

The dimensions which I have found sufficient for large works are as follow:

The bore of the exhausting cylinder two feet.

The length six feet, so as to afford a stroke of four feet.

The

The pipes which conduct the air to such an engine ought not to be less than six-inch bore.

The best rate of working is from two to three strokes a minute; but if required to go much faster it will be proper to adapt a capacious air-vessel to the pipes near the machine, which will equalize the current pressing through them.

Such an engine discharges more than two hundred gallons of air in a minute; and I have found that a stream of water supplied by an inch and a half bore falling twelve feet, is sufficient to keep it regularly working.

A small engine to pump out two gallons at a stroke, which would be sufficient in many cases, could be worked by a power equal to raising a very few pounds weight, as the whole machine may be put into complete equilibrium before it begins to work, and there is hardly any other friction to overcome but that of the air passing through the pipes. Small engine.

The end of the tunnel of the Tavistock Canal, which it was my object to ventilate, was driven into the hill to a distance of near three hundred yards from any opening to the surface, and being at a depth of one hundred and twenty yards, and all in hard schistus rock, air-shafts would have been attended with an enormous expense; so that the tunnel being a long one, it was most desirable to sink as few as possible, and of course at considerable distances from each other. Thus a ventilating machine was required, which should act with sufficient force through a length of near half a mile, and on the side of the hill where it first became necessary to apply it, no larger stream of water to give it motion could be relied on, than such a one as I have mentioned after the description of the engines; and even that flowed at a distance from the shaft where the engine was to be fixed, which made a considerable length of connexion rods necessary. Ventilator applied to the tunnel of the Tavistock canal.

Within a very short time after the engine began to work, the superiority of its action over those formerly employed was abundantly evident. The whole extent of the tunnel, which had been uninterruptedly clouded with smoke for some months before, and which the air that was forced in never could drive out, now became speedily so clear, that the Its action.

day

day light and even objects at its mouth were distinctly seen from its farthest end. After blowing up the rock, the miners could instantly return to the place where they were employed, unimpeded by the smoke, of which no appearance would remain underground in a very few minutes, while it might be seen to be discharged in gusts from the valve at the top of the shaft. The constant current into the pipe at the same time effectually prevented the accumulation of air unfit for respiration. The influx of air, from the level into the mouth of the pipe, rushes with such force as instantly to extinguish the flame of a large candle; and any substance applied, so as to stop the orifice, is held tight by the outward pressure.

It is now more than two years since the machine was erected, and it has been uninterruptedly at work ever since, and without repair. The length of the tunnel has been nearly doubled, and the pipes of course in the same proportion, and no want of ventilation is yet perceptible.

Two similar engines have been since constructed for other parts of the same tunnel, and have in every respect answered the purpose for which they were designed.

The original one is worked by the small stream of water before-mentioned, by means of a light overshot-wheel twelve feet in diameter, and about six inches in breast.—The two others are attached to the great overshot-wheel, which pumps the water from the shafts which are sinking upon the line, and as their friction is comparatively nothing, this may be done in any case, with so little waste of power for this purpose as not to be an object of consideration, even if the power be derived from more expensive means.

Its application to various purposes.

The size of the exhauster may always be proportioned to the demand for air, and by a due consideration of this circumstance, this engine may be effectually adapted not only to mines and collieries, but also to manufactories, workhouses, hospitals, prisons, ships, and so on.

Thus, if it were required to ventilate a shaft of a mine, or a single level, which is most frequently the case, where three men are at work at one time, and we allow that those three men vitiate each twenty-seven cubic inches and a half of air per minute, (as determined by the experiments of Messrs.

Messrs. Allen and Pepys); and allowing farther, that their candles vitiate as much as the men, there will be six times twenty-seven cubic inches and a half of air to be drawn out in a minute, equal to one hundred and sixty-five.

Now a cylinder five inches in diameter, working with a stroke of nine inches, will effect this by one stroke in a minute, though it would certainly be advisable to make it larger.

Not being practically acquainted with collieries, or mines that suffer from peculiar gasses that are produced in them, I cannot state, from actual experiment, what effect this machine might have in relieving them; but it must appear, I conceive, evident to every person at all acquainted with the first principles of pneumatics, that it must do all that can be wished; as it is obvious, that such a machine must in a given time pump out the whole volume of air contained in a given space, and thus change an impure atmosphere for a better one. And in constructing the machine it is only necessary to estimate the volume of gas produced in a certain time, or the capacity of the whole space to be ventilated. It is easy to judge how much more this must do for such cases as these, than such schemes as have lately been proposed of exciting jets of water, or slacking lime, both of which projects, likewise, must fail when applied; as one of them has, I believe, been proposed to be to the case of hidrogen gas. But with such a machine as this, if the dreadful effects of explosions of this air are to be counteracted, it may be done by one of sufficient size to draw off this air as fast as it is generated; and by carrying the pipes into the elevated parts of the mine, where from its lightness it would collect. If, on the other hand, it is desired to free any subterraneous work from the carbonic acid gas, it may as certainly be done by suffering the pipe to terminate in the lower parts, whither this air would be directed by its gravity.

Its application to collieries; and to fire-damp, or choke-damp:

In workhouses, hospitals, manufactories, &c., it is always easy to calculate the quantity of air contained in any room, or number of rooms, and easy to estimate how often it is desirable to change this in a certain number of hours, and to adjust the size and velocity of the engine accordingly. Where this change of foul air for pure is to take place in the

to workhouses, hospitals, manufactories, &c.

ht, means for working the machine may be provided by pumping up a quantity of water into a reservoir of sufficient height to admit of its flowing out during the night in a small stream, with sufficient fall, so as to give motion to th gine; or by winding up a weight of sufficient size; o nany other means, which are easily devised.

'or instance, a room in which fifty persons slept was y feet long, twenty wide, and ten high, it would con- 16000 cubic feet of air; and if this was to be removed twice in eight hours, it would require a cylinder of thirty inches diameter, working with a four-foot stroke four times in a minute, to do it; or nearly that. Such a cylinder could be worked by the descent of ten gallons of water ten feet in a minute; or, for the whole time, by eighty hogsheads falling the same height.

But this is a vast deal more than could be required, as the fifty people would, in eight hours, only vitiate three thousand gallons of air, which could be removed by one hundred and fifty strokes of a cylinder, twelve inches diameter, with a four-feet stroke, which would not require an expenditure of more than one thousand five hundred gallons of water properly applied, or about twenty-eight hogsheads.

JOHN TAYLOR.

Holwell, near Tavistock,
Feb. 7, 1810.

CERTIFICATE.

Testimony of its efficacy.

An extract from the Report of the Committee of Management of the Tavistock Canal, to the General Meeting of Proprietors, held in August 1808, stating, that great impediments had arisen from the want of good air in the tunnel when distant from a shaft, then adds—" For the purpose of rendering the ventilation in the tunnel completely good, and of doing it in a manner that may be applied to very considerable lengths in driving, the engineer has erected machines, acting upon the simplest principle, and without friction, which exhaust from the very place in which the men are working a continued volume of vitiated air; the place of which, of course, is as constantly supplied with fresh air, by the pressure of the atmosphere, and thus all difficulty on this head completely ceases."

V. On

V.

On the Processes employed to cause Writing to disappear from Paper, to detect the Writing that has been substituted, and to revive that which has been made to disappear; Improvement of common Ink; a Notice of a new Ink, that resists the Action of chemical Agents: by B. H. TARRY, *M. D.* *.

WRITING is removed either by scraping with a knife, or by means of acids. When writing has been scratched out, commonly pounce, or size, is applied to the paper, that the ink afterward used may not run. If pounce have been employed, the strokes of the same pen will appear more slender, if size more full, than on other parts of the paper. Immersion in warm water for a few minutes will dissolve and wash away size: alcohol will have the same effect on pounce. After the paper is taken out, it should be dried slowly; at first in the shade, till three parts dry, and afterward between the leaves of a book, or a quire of paper. While it is drying the ink last used will spread and sink into the paper more or less. Generally indeed close inspection with a good lens will show where any writing has been scratched out, by the appearance of some loose or torn filaments. Indications of writing having been scratched out.

If the means employed to obliterate writing have been such as to remove the whole of the iron from the paper, every attempt to restore the writing must be vain. If some ferruginous compound remain, the characters may be reproduced in their original form; though the colour will vary, according to the nature of the compound in which the iron is concealed, and of the reagent employed. If all the iron have been removed the writing cannot be restored.

In some cases the gallic acid is capable of recomposing the writing, that has been made to disappear by chemical means; but its attraction for the oxide of iron is not so strong as is commonly supposed. The red or brown oxide of iron, obtained from the sulphate or nitrate by means of Sometimes it may by the gallic acid,

* Abridged from the Ann. de Chim. vol. lxxiv, p. 153; and from the report made to the Institute by Berthollet, Vauquelin, and Deyeux, ib. vol. lxxv, p. 194.

 alkaline

alkaline carbonates, cannot combine with the gallic acid to form ink, unless the carbonic acid have been expelled from the oxide of iron by some more potent acid. It is the same with respect to the oxalic acid, and acidulous oxalate of potash: when this acid or this acidulous salt has seized the oxide of iron, the gallic acid cannot destroy the combination, because it has an inferior attraction for the oxide of iron.

If the writing have been destroyed by nitric or oximuriatic acid, the gallic acid in tincture, infusion, or decoction of galls will revive it.

prussiate of lime or potash,

Liquid prussiate of lime or potash is a good reagent, to detect the presence of iron. If the ink have disappeared in consequence of the decomposition of gallic acid, as when oximuriatic acid has been employed, either of these will render it legible, causing it to appear of a light greenish blue while wet. If oxalic acid have been employed to obliterate the writing, the prussiates will restore it of a reddish brown colour. If nitric or sulphuric acid have been employed, the prussiate of lime will show this by staining the paper blue, but it cannot reproduce the writing.

hidrogurretted alkaline sulphurets.

Hidrogurctted sulphurets of the alkalis, or of the alkaline earths, are very prompt and powerful tests of ferruginous salts. The alkali, or earth, combines with the acid; and the sulphuretted hidrogen with the oxide of iron, forming an hidrogurctted sulphuret of iron. Iron in the state of red oxide is partly disoxidated by the hidrogen, water is formed, and the iron passes to the state of black oxide. This is the case with writing turned rusty: these reagents immediately change it to a green black, much deeper than gallic acid would give. A solution of sulphate of iron mixed with an hidrogurctted sulphuret produces a very deep green black ink.

The same attractions are exerted when the hidrogurctted tests are applied where writing has been obliterated by the oxalic acidule or the oximuriatic or nitric acid. If the oxalic acidule were employed, the characters will reappear of a green black or brown red. If the oximuriatic acid, of a green black or pale rust colour. The less the revived writing approaches a black, the more the iron was oxided

in the metallic salt decomposed, or the less the iron was disoxided by hidrogen. The writing on which nitric acid has acted strongly cannot be reproduced: but on passing sulphuretted hidrogen over the paper where it was, waving lines of a green black will be formed on the remotest parts to which the sulphuretted hidrogen penetrates. These lines may be produced in great number, and in different directions. They are owing to the sulphuretted hidrogen combining with the oxide of the ferruginous nitrate. If the undulating lines, or the letters that have been restored, should disappear, they may be reproduced by dipping the paper into cold water. Beside the traces of writing, and the undulating lines just mentioned, the paper takes a yellow colour when it is not impregnated with an acid, and a green more or less deep when it is. The green colour will be deeper, in proportion as the acid was stronger, or in larger quantity. In all cases the paper retains the colour of fresh butter after it is dry. The hidroguretted sulphurets should be diluted with half or two thirds their quantity of water before they are used, as in their ordinary state they are too strong.

Indications of writing obliterated by nitric acid.

From what has been said, we may hope to restore writing, that has been obliterated by any agent except the nitric acid: and if this have been employed only in small quantity, without the assistance of any other acid, and its action has not been too long continued, on holding the paper to the fire the writing will reappear of a rust colour.

Method of restoring it when practicable.

With regard to the improvement of ink, little progress has been made since the time of Lewis. Inks made by infusion, and with green sulphate of iron, are of a Prussian blue colour, light, pale when written with, but growing black as they dry on the paper. Those made by decoction are blacker, thicker, and form a more copious sediment, which is of a dirty Prussian blue colour. Decoction extracts from galls all the soluble parts; infusion takes up chiefly the gallic acid, and mucilage, with a little extract and tannin. In the decoction the iron of the green sulphate becomes more oxided, and the extract and tannin acquire oxigen, by absorption from the atmosphere; and the iron in a higher state of oxidation, and the oxigenized extract, produce a deeper black with the gallic acid and tannin.

Improvement of ink.

The

ore abundant sediment is owing to a larger quantity of extract and tannate of iron. In inks made by infusion, the oxide of iron, extract, and tannin, increase their oxigenation very little, till they come to dry on paper. Nitric acid immediately obliterates writing with ink made by infusion, but that which has been made by decoction resists its action much longer, on account of the larger quantity of extract in it.

Infusion or decoction of galls should be kept some time.

In proportion as the infusion or decoction of galls grows old, its surface is covered with mother, which is the mucilaginous principle separated. This mother ceases to form in about a year, during which the pellicle produced on the surface should be removed three or four times. The infusion or decoction of galls grows brown as it becomes oxigenized, takes an amber colour, and emits a pleasing smell; and, when combined with green sulphate of iron, no longer produces a Prussian blue, but a green black. The amber colour of this infusion or decoction is owing to the oxigenized extract and tannin. The green colour of the ink arises from the mixture of the black of the gallate of iron with the fawn colour of the oxigenized tannin, which in this state can no longer combine with the oxide of iron. If the tannin be separated from the infusion or decoction by means of an alkali, the green or red sulphate of iron forms with it a very black and purer ink; and the alkali in the solution facilitates the union of the oxide of iron with the gallic acid, by combining with the sulphuric acid of the sulphate. The oxigenized extract concurs in rendering the ink blacker, as does the oxide of iron more highly oxided.

Infusion preferable.

Infusion of galls is preferable to the decoction, as it dissolves the principle, that is essential to the composition, and very little of those that are foreign to it. Logwood browns the ink, and loads it with its colour; it is better therefore, to use in its stead a small quantity of galls in addition to that directed by Lewis. The following is the composition of a good ink.

Receipt for good ink.

Infuse in one litre [a wine quart] of rain or river water 125 gram. [4 oz. troy] of bruised galls, letting them stand in the sun four hours in summer, or six hours in winter. This infusion may be used immediately after straining; but it

it is better to let it stand four or six months, removing the mother that forms on the top now and then, and finally separating by filtration both this and the tannin that has fallen to the bottom. In this dissolve 32 gr. [a troy ounce] of powdered gum arabic; then add the same weight of finely powdered sulphate of iron, superoxigenized by calcining it till it grows reddish; and continue shaking the mixture till this is completely dissolved. The ink thus made is fine, light, and of a purple tinge, but black when dried on the paper. It is nearly, if not precisely, the composition of Guyot's ink.

Indelible ink.

Dr. Tarry next proceeds to his indelible ink, the composition of which however he does not disclose. He says only, that it contains neither galls, nor logwood, nor brazil, nor gum, nor any preparation of iron; that it is entirely vegetable; and that it resists the action of the most powerful acids, of alkaline solutions in their most concentrated state, and of all solvents. He sells it in a solid form; and for use it is to be mixed accurately in a mortar with eight parts of water, and then put into a bottle left at least one third empty, for the purpose of shaking it, which is to be done every six or eight hours for a couple of days. It soon soften quills, but metallic pens are well adapted to it, as it contains no acid. There is no danger from putting the pen into the mouth, as it contains nothing deleterious.

Action of acids and alkalis on it.

Nitric acid has very little action on this ink. Oximuriatic acid only changes it to the colour of goose dung. After it has been acted on by this acid, caustic alkaline solutions give it the colour of carburet of iron. The letters however still remain unchanged in form, and these effects require a long maceration for their production.

Report of the committee.

From the report of the committee it appears, that the ink of Dr. Tarry possesses the properties he ascribes to it; but they add, it has one of the faults common to all the indelible inks proposed, that of pretty quickly forming a considerable sediment, which deprives the supernatant fluid of its properties, so that it requires to be shaken every time it is used.

VI. On

VI.

On the Sense of Smell in Fishes: By M. C. Duméril*.

Holes in the heads of fishes called nostrils.

ALMOST all the fishes hitherto observed have nostrils †. At least this name is given to two deep holes, which are generally found in the heads of these animals between their eyes and lips. These cavities have a single slender orifice; and within they are lined with a mucous membrane, having numerous folds. The first pair of nerves from the brain enter into the substance of this membrane, ramify in it, and there terminate. Analogy therefore seems to indicate, that the nostrils of fishes are particularly intended for the organ of smell, as in all other animals with vertebræ. Against this opinion however, adopted by all naturalists and physiologists, I have some facts and reflections to offer, which perhaps will seem more consistent with our knowledge in comparative anatomy and physiology.

Supposed to be the organ of smell: but this questionable.

Fundamental propositions.

I propose to show, that the organ of smell does not and cannot exist in the mouths of fishes, from their manner of breathing: that the organs, hitherto considered as adapted to the sense of smell in these animals, are intended for the perception of a sensation analogous to that of taste; and

[redacted]

determine, whether the sensation be imparted through the medium of the lingual branch of the fifth pair, that of the glossopharyngean, or that of the great hypoglossal nerve.

The general opinion in favour of a branch of the lower maxillary nerve.

It is true the majority agree in considering the lingual branch of the inferior maxillary nerve as the only one capable of transmitting the sensation of taste; and most of them adduce in support of their opinion the observation of Colombo, who did not find this branch in a man destitute of the sense of taste. Soemmering, however, questions the circumstances of this fact, as well as of a similar one cited by Rolfink.

Others for the great hypoglossal nerve.

On the other hand some physiologists, at the head of whom is the great Boerhaave, have ascribed the gustatory faculty to the great hypoglossal nerve. These too rest their opinion on some anatomical observations, particularly on a case in pathology quoted by Hevermann, where the sense of taste was destroyed on the extirpation of a gland, with which the nerves, called at that time the great gustatory, or ninth pair, were removed.

The particular subject of physiology and comparative anatomy before us, therefore, may throw some light on a question not yet completely resolved.

The sense of taste necessary to animals:

Though the sense of taste is essentially necessary to animals, and must be the last obliterated, since on its decisions depend their preservation, by instructing them in the nature of the substances proper for their food, and the selection of them; at first sight, however, it would appear, that fish are destitute of it, if we seek for this organ in the parts where it is commonly seated.

but fishes apparently destitute of it;

as it cannot reside in their mouth,

In fact the inside of the mouth in fishes is lined with a thick, smooth, and polished membrane; of a very close texture, resembling that of the skin; and most commonly of the same colour with it. Sometimes this membrane is completely detached from the bones of the palate, or retained merely by a few vessels; as I have observed in the cod, frogfish, bullhead, ray, and shark: and I have never seen in it papillæ, or salivary glands.

or tongue.

The tongue of fishes is seldom movable. A bone supports it throughout its whole length. Its point can neither turn backward, nor toward the sides. In general the lips,

palate,

tongue, and branchiostegous rays are covered with y ints, or laminæ of different forms, which prevent in iate contact of substances taken into the mouth. It is true n the muscles of the hyoides and of the branchios-te ays, placed at the lower part of the mouth, we find a amifications of the nerves of the fifth pair, as well as f the indeterminate nerve, which evidently has the pla f the glossopharyngean. Yet neither I nor Mr. Cuvi could ever meet with the great hypoglossal nerve in fish, notwithstanding our most attentive searches, when I enj d the advantage of editing his lectures on comparative anatomy. — is fact was of great importance to the sub sent paper, I think it proper to add, that I ed myself of it by fresh anatomical researc

T p w then

The sensation of the mouth deadened,

It is easy to imagine, that the water, by its continual entrance into the mouth, and the compression it there undergoes, as often as the fish exerts on it the action of deglutition necessary to force it through the gills, must exert a friction so often repeated, as to deaden all the sensibility of these parts.

and the organ of taste cannot exist in it.

Now since the integuments of the inside of the mouth are coriaceous, destitute of salivary glands, and frequently roughened with teeth or horny points; the tongue adherent, bony, and immovable; the great hypoglossal nerve wanting; and water continually exerting a friction on it: it is very probable, that the organ of taste cannot exist there. This was the first point I proposed to examine.

Probably it is in some other part.

As the organ of taste appears not to reside in the mouths of fishes, and this sense is indispensable to animals, we must meet with it elsewhere: and since tastes in general bear a considerable analogy to smells, let us inquire whether the sense of smell be not to a certain degree converted into that of taste. But, before we enter on this investigation, let us examine the nature of these two sensations.

Nature of smells,

Natural philosophers, chemists, and subsequently physiologists, have generally attached to the idea of smell, that of the sensible existence of corporeal atoms of extreme minuteness. Though art has not yet been able to imitate an instrument so perfect as that met with at the entrance of the respiratory

respiratory organ in animals that live in the air, we have some means of proving chemically the material existence of those smells, the nature of which is best known. Thus the exhalations from nitrous gas, volatile oils, and ether, for example, may be destroyed by the combination of some of their principles with oxigen; and muriatic acid gas renders sensible to the eye the particles of ammonia, which cease to be odorate the moment this acid combines with them in the open air.

Proofs of their materiality.

The most perfect animals, those that possess all the five senses, are so organized as to perceive the principal modifications of the bodies surrounding them. They have sight, to enjoy the effects of light; feeling, to appreciate the solidity of palpable objects; hearing, to distinguish the vibrations of elastic bodies; taste, to discriminate the qualities of bodies capable of becoming liquid; and lastly smell, to collect the emanations of substances, that have the properties of a gas.

Senses of perfect animals.

Light exerts its action only on the eye; not on the tongue, nostrils, ears, or skin. It is the same with most smells, which do not act on the sight, taste, hearing, or touch. Each of the organs of sense then has its particular function, fixed and determined beforehand by the arrangement of its apparatus: for the sentient principle appears to be identical, and placed, as we may say, on the watch on the inside of each instrument, in order to collect and transmit the slightest modifications in the qualities of bodies.

Each sense has its peculiar object, dependent on the organ, as the sentient principle is one.

The sensations of smell and taste however, are most analogous, both in respect to the mode of action on our bodies, and to the apparent end at least for which nature seems to have given us organs to perceive them. The odorate and sapid particles are conveyed either by the airs that serve for respiration, or the solid and liquid aliment that must enter the stomach. Stopped on their passage through the nostrils or the mouth, these particles touch the nerves distributed on those parts, and thus give notice of their presence. The nerves immediately excite the ideas of the sensations they perceive, and excite us to admit or reject the air or food, according as the impression produced on the organ is agreeable or not. The sapid and odorate qualities of bodies then

Smell and taste have considerable analogy.

are

are discriminated by the tongue, when they are contained in a liquid; and by the pituitary membrane, when they are suspended in a gas.

Smell peculiar to the state of gas,

From these general considerations of the nature of smells and tastes, it appears, that liquids cannot intrinsically possess smell, since this quality of bodies is inherent in their state of gas, or vapour. We are justified therefore in presuming, that an animal, which from its nature must be immersed in a liquid all its life, does not possess a sense of which it can make no use: and this is the case with cetaceous animals, fishes, most of the mollusca, a great number of crustaceous animals and worms, and all the zoophytes.

and cannot be perceived in a liquid.

Cetaceous tribe analogous to fishes in their mode of respiration;

In a former paper I have pointed out the analogy between fishes and cetaceous animals, with regard to the mechanism of respiration*. It is in consequence of this mode of respiration, if I may so say, and of their necessary abode in water, that the organ of smell appears to be annihilated in these animals; for as Daniel Major and John Hunter first observed, though only in a few species, and Cuvier has since shown generally and more at large, there are no olfactory nerves, and no ethmoidal foramina, in the cetaceous animals. The pituitary membrane, that lines their nostrils, is smooth, dry, and coriaceous: it appears to have become insensible from the constant friction on it occasioned by the rapid and violent action of the water, that pervades the cavity of the nostrils. It appears however, that the organ of taste here supplies the place of that of smell; for, by a slight modification of the organs, the olfactory nerves of fishes may have another use, and be destined to make them sensible of tastes.

and want the olfactory nerves.

These nerves have another use in fishes.

Though fishes cannot smell,

From the ideas we have formed of the nature of smells, it necessarily follows, that fishes cannot receive impressions similar to those they occasion in animals that breathe air. Yet we know, that fishes are attracted by the emanations, that escape from several substances immersed in water, as is demonstrated by various baits employed in fishing; the salted roes of cod and mackarel, the broiled or stinking flesh of certain animals, old cheese, and many other things of strong smell.

they are sensible of emanations from substances.

* See Journal, vol. xxviii, p. 355.

Aristotle

Aristotle was acquainted with most of these facts, and even recites them at large in his History of Animals: yet he says positively, "fishes have no distinct organ of smell, for there is but one orifice to the apertures they have in the place of the nostrils." And elsewhere, "we see in them no external organ of hearing or smell, not even an aperture." Mr. Schneider, in his Synonimes of Artedi's Fishes, reproaches Aristotle with entertaining this opinion, after having so well described the olfactory organ and nerves in these animals. It is in some measure therefore a defence of Aristotle's opinion, if I endeavour to show, that every emanation in water must produce on the nerves, with which it comes into contact, a sensation analogous to that of taste.

This known to Aristotle,

whose opinion is here defended.

Since there are no real smells in water, the exhalations, that escape from bodies immersed in it, either rise to the surface in the form of gas, and consequently do not remain in the liquid; or they are suspended in it or combined with it, and they participate in all the properties of liquids. If however the qualities of these particles, thus dissolved, be perceptible, they necessarily come under the same circumstances as sapid bodies; and therefore it would be useless for fishes, which live habitually in water, to be endowed with the organ of smell.

The organ of smell would be useless to fishes.

To prove the accuracy of this reasoning, it is necessary to investigate the use of the nervous apparatus, which has hitherto been supposed to be intended for the perception of smells: and to this I shall proceed, treating it more minutely than in the beginning of this paper.

Use of the nervous apparatus supposed to be intended for smelling.

The cavities termed nasal are always situate before the eyes, in the space between the nasal bones and those of the upper lip. Sometimes they are in the substance of the bones of the nose themselves, or between these and the pieces which Artedi has called hypophthalmic. The heterosome fishes, as the pleuronectes, the only animals with vertebræ that are not symmetrical, are the only ones that have both nostrils on one side of the body, in some on the right, in others on the left, and unequal. Lastly, though most of these species have these cavities on the top of the head, in the forehead; they are found beneath, and most frequently

The nasal cavities described.

frequently communicating with the mouth, in all the plagiostomes, as the ray, the shark, &c.

In all fishes these cavities present a kind of sinus, or cul-de-sac with a narrow opening; most commonly divided into two portions, sometimes into three, as in the eel, by a membranous septum, variously convoluted, which icthyologists have frequently noticed as characteristic of species.

We know from the observations of Monro, that these valves or curtains may be moved according to the will of the animal; and that under certain circumstances the orifice may be nearly covered by the septum. It is easy to observe this in live fishes, as I have seen in the goldfish and stickleback. It is then apparent, that the motion of the septum seems to be the consequence of the protraction of the lips; since at each inspiration the cavity opens and dilates, while it contracts and is covered as often as the mouth is closed: whence it seems to follow, that at every inspiration the fish causes a small quantity of water to enter on each side, which it may be said thus to analyse.

The first pair of nerves and part of the 5th spread on the membrane within them.

Each of these perforations exhibits within a cavity, very spacious in proportion to its orifice; and on this is spread the sentient membrane covered with mucus, in the substance of which is expended the whole of the first pair of cerebral nerves, and one or more large branches of the fifth pair, according to the observation of Collins quoted and corrected by Cuvier.

These cavities are always separated from the respiratory canal.

Nor must I forget to remark, as a circumstance particularly deserving notice, that these pretended nasal cavities are always separated from the canal of respiration; and that it is only in the rays, and some neighbouring genera, which have spiracles, that they are observed almost in the mouth. In fact it is to be presumed, that the liquid, in traversing them, would have deadened the sensibility of their surface by the rapidity of its motion, and the friction of its particles.

What are the inferences from this structure?

Now are these peculiarities of structure, which I have mentioned, of such a kind as to lead us to abandon our first opinion, deduced from the knowledge of physics, that smells cannot be perceived in water? or is not this supposed organ of smell in fishes better adapted to excite in them the sensation

sensation of tastes? These questions I shall proceed to examine.

Tastes and smells analogous.

Tastes and smells are nearly of the same nature: both sensations are produced by the physical and chemical qualities of bodies. We know, in fact, that very minute particles are continually separating from certain substances, which, without being decomposed, come to act immediately upon animals at that point of their surface alone, where they can manifest their presence. This phenomenon is effected by the intervention of a fluid medium, and a sort of contact*.

The organs in question perfectly adapted to the sense of taste.

All the conditions necessary for the impression or sensation of taste are united therefore in the organ under examination, and the nature of the substances that may produce it. First, the organ is placed secure in a cavity: it opens and shuts at the will of the animal, it admits or rejects emanations at pleasure. Secondly, the sentient surface receives numerous and bulky nerves from the fifth pair; it is soft, moist, and mucous; and it presents a great surface in a large space. Thirdly, it appears in a certain degree to supply the place of the organ of taste, which cannot exist in the mouth of fishes from the very mechanism of their respiration.

General conclusions.

It seems to follow then from all these circumstances, that the organ of taste in fishes does not reside in the mouth: that the sensation of taste is probably imparted to them by the apparatus, which has hitherto been considered as adapted to perceive the emanations of odorate bodies: and lastly, that no real smell can be perceived in water.

* I have already had occasion to enlarge on these general ideas in a paper on the organ of smell in insects, which I published ten years ago, and which may be found in the second volume of the Magazin Encyclopédique, p. 435.

VII. On

VII.

On the Alum Mines of Aubin in the Department of the Aveyron; by Mr. L. Cordier, *Engineer in chief, &c.* *

Alum mines of Aubin.

THE alum mines of the country of Aubin differ from those of the same nature worked in other places: their existence is wholly contingent. The periods of their formation are known, and are very recent. They occupy no considerable space of ground, and cannot extend much farther. Lastly their duration must be very limited, whether they be worked or not. These mines are nothing but coalpits, that have taken fire within a certain distance of time, and in which the fire is still daily exercising its ravages. There are four of them; those of Lassale, Fontaines, Buégne, and Bourlhones. To give an idea of their situation, that of the coal in the country must be known.

from coal-pits that have taken fire.

Description of the coal country.

The territory of Aubin is very hilly, and intersected by deep narrow passes. The part to the north-east of the town consists entirely of coal country, and is the least elevated, being nearly in the form of an elliptic basin, the great axis of which is north and south, and the surface of which exceeds a square myriametre [24676 acres]. This space is skirted and overtopped on all sides by the primitive soil; and is occupied by a pretty considerable number of oblong hills, intersected in every direction, and crowded together. The highest, for they are unequal in this respect, are two or three hundred metres above the valleys.

Its structure.

The arrangement of the strata throughout the basin exhibits nothing constant, or continued. Independent of the interruptions occasioned by the narrow passes and valleys, the direction, inclination, and order of the strata vary from one hill to another; so that to depict the present state of the soil, it is sufficient to say, that it appears to be the result of a complete disruption. We can merely perceive, that the directions more frequently approach the meridian than any other line, and that the prolongation of the

* Abridged from the Journal des Mines, vol. xxvi, p. 401. From the Report made to the council of Mines in 1807.

strata

strata is almost always in the longitudinal direction of the hills. As to their dip the strata are generally set on edge: they hang in all directions, and at every possible angle from perpendicular to horizontal: the strata of two neighbouring hills are seldom seen to incline the same way; and, when this does occur, it is at different angles. The hills nearest together offer striking varieties, and frequently singular for the nature, order, and thickness of the strata. It is even in vain to seek for some similarity of structure in certain places, where the strata that skirt a valley are placed so, that they would come to rest against the strata on the other side, if both were sufficiently prolonged. Hence it may be conjectured, not only that the surface has been completely broken up, but that it has experienced considerable degradations subsequent to this disruption.

The coal ground is almost wholly formed of a gray sandstone, commoly fine-grained, and composed of feldspar, quartz, and some particles of mica. The mean thickness of the strata is about a yard: some are found, that are more than ten yards thick, others not a tenth of a yard. In the midst of these sandstones are seen thick strata of puddingstones with granitic fragments; and strata, generally thin, of gray or blackish argillaceous schist exhibiting some impressions of vegetables. Coal is found throughout almost the whole of the basin. The outcroppings are very numerous, and occur indiscriminately at the foot, on the acclivity, toward the summit, or on the ridges of the hills. The number of the seams, their thickness, and their distance from each other, vary in every hill. They are almost all thick enough to be worked. There are seldom more than four in one hill. Their mean thickness is in general from two to six yards; but in some places it is truly astonishing, and hitherto unexampled. The vertical seam now working at Lassalle is 103 met. [338 feet]. Its course is perfectly regular, and known, for it is worked by means of levels extending from the roof to the wall.

Strata.

The coal.

From what has been said it is obvious, that the coal of this country is as easy to extract as it is abundant. It is worked in fact in a number of places, and almost every where by means of levels. The coal is embarked on the river

Management of it.

river Lot, which runs near the mines. But this union of natural advantages, far from being turned to profit by good management, has hitherto given rise to various abuses; though I shall only point out that, which relates to the subject of the present paper. From time immemorial every landholder has been at liberty to dig in his own ground, get out the coal without order or method, and dispose of it as he could. Hence the number of pits opened has had no reference to the demand, and frequently individuals have been obliged to relinquish their works for want of a sale for their produce. Now from causes which it is useless to discuss here *, the works that are thus given up are liable to take fire spontaneously, even when carefully watched. The fire communicates very rapidly every where; and if the greatest exertion be not made to stop it in the beginning, it becomes afterward impossible to check its ravages, and the work is destroyed. It appears, that this misfortune happened very often formerly; for, on going over the surface of the ground occupied by the mines, at almost every step we meet with very evident traces of subterraneous fires now extinct. Accidents of this kind are now more rare, either from the people having learned how to prevent them, or knowing how to check them: yet seven or eight works are still burning at this moment.

The works that have been relinquished take fire spontaneously.

Among these works that have caught fire, those called Lassalle, Fontaines, Buégne, and Bourlhones, are the only ones remarkable, either on account of the intensity of the fire and the space it occupies, the disruption and torrefaction of the earth as far as the surface, or the daily pro-

Four of these only deserve notice.

* In general only the purest coal is got out of the works. That which is mixed with schist, being of no value, it is used with other matters to fill up the vacuities made. Now whether this be frequently accompanied with iron pyrites disseminated in it, or perhaps even contain sulphur in combination, the fact is, that moisture renders it a very active pyrophorus, in all parts of the mines where the circulation of the air is stopped. The miners of the country have but one opinion on the subject: they all agree, that the spontaneous inflammation of the works is owing to the action of stagnant water on the refuse left in them; and that the fire manifests itself the more speedily, in proportion as the circulation of the air is more slow.

Spontaneous combustion of the coal.

duction

duction of a considerable quantity of aluminous salts amid the torrefied rocks.

The pits destroyed, but compensated by the alum.

The burning pits, whence the alum works originate, must be considered as totally destroyed; but the alum produced will more than compensate for the loss of the coal. It is known too, that the fire will go out of itself, as soon as it has consumed all the masses of fuel, that have been exposed by the levels. It has long been ascertained, that the fire does not extend more than a yard into the coal left untouched in depth. This is so certain, that the extraction of the coal from beneath the works burned has been resumed at Lassalle and Fontaines.

The fire does not burn more than a yard deep.

Effects of the combustion.

The effects of the spontaneous combustion of the coal are the same in the four alum works. To judge from the state of the surface of the earth, the fire has not extended beyond the space that had been worked. The surface has sunk, cracked, and been deranged, in the manner of volcanic solfaterras. It emits a gentle heat, incessantly renewed; it is bestrewed with very curious productions of fusion and torrefaction; the crevices emit burning fumes of sulphurous acid, bitumen, and water; and even flames continually arise when the fire is consuming a stratum near the surface. The sandstones and schists, that accompany the burning seams of coals, are either simply torrefied, or changed into red, light, and rugged scoriæ, or violet-coloured, bluish, gray, and often striped, enamels. The acidosulphurous vapours attack, deprive of colour, and decompose part of these products, and frequently reduce them to powder; and at their expense are formed the vitriolic saline substances, that are found in such great abundance, either in the cavities of the masses, or amid the earth resulting from their decomposition, or on the surface of the ground. The simple or alkaline * sulphate of alumine constitute almost the whole of these saline substances. They exhibit themselves in all forms; in disseminated particles, discoverable only by their acerb and styptic taste, in whitish efflorescences, and filamentous and

* The alkali is probably furnished either by the combustion of the coal, or the decomposition of the feldspar, which abounds in the rocks affected by the acidosulphurous vapours.

silky masses, in yellowish mamillary incrustations, or in confused masses, friable, cavernous, white, gray, yellow, red, or a mixture of all these colours. It may not be superfluous to add, that the last variety is sometimes met with in blocks or incrustations weighing several pounds.

Such are the general characters of these alum mines, but there are particular ones, which ought to be noticed.

The mine of Lassalle.

The alum mine of Lassalle is in the bottom of a valley, at the foot of the hill of the same name, two miles north by west from the town of Aubin. The surface it occupies on a slope of about 45°, does not amount to 2 hect. [247 acres]. The subterranean fire has not exceeded the limits of the coal-pit: it occupies the length of 250 met. [273 yards] at the foot of the mountain, and extends nearly 70 met. [76 yards] into it. It has attacked nothing below the level of the brook, that flows through the valley.

Burning these twenty years.

This pit took fire spontaneously about twenty years ago. The stratum of coal, which feeds it, was three or four yards thick, and worked by means of levels. Attempts were made to extinguish the fire at the time, but in vain. The inclination of the strata in this part of the mountain is about 8° or 10° W. N. W.; or contrary to the slope of the mountain.

The fire now abating.

The activity of the fire has decreased greatly within these few years. It appears to be drawing to an end; or that the accumulation of torrefied and decomposed substances, that cover the surface, has retarded its ravages. The effect of the exavations made within these six months seems to confirm the latter opinion. Vapours now issue out abundantly by all the new vents they have been able to make, and the saline efflorescences increase more rapidly.

This mine has not been worked above nine months.

Mine of Fontaines.

The alum mine of Fontaines is at the bottom of the cul-de-sac, that terminates the valley of Lassalle, and at the foot of the mountain 2500 met. [2732 yards] N. E. of Aubin. It takes its name from a hamlet directly above it. Its surface is nearly square, and may be 3 hect. [370 acres]. The foot of the mountain at this place has a slope of about 50°.

Has burned these 80 years in different seams.

The fire commenced here eighty years ago. Several seams of coal were then working, one over another, and inclining

inclining 35° or 40° W. S. W. It was got out by means of levels, and with so much the more ease as the mountain slopes to the north. Each seam having been worked in several places, and to some distance, as 80 or 100 met., the fire has made more ravage, than at either of the other three places. Notwithstanding the length of time, the activity of the fire has not abated, at least in the higher parts. In fact we see there the sunken surface of the earth intersected by long and deep fissures, the sides of which are in the highest state of incandescence, and from which flames, accompanied with suffocating vapours, are continually escaping. In a word, the solfaterra of Fontaines presents the most curious combination imaginable of all the phenomena above described*.

The vitrified, scorified, and decomposed matters, that fill the space occupied or traversed by the fire, are very rich in aluminous salts.

The alum mine of Buégne is at the top and on the back of the hill of Buégne to the east. It is about 2 kilom. [1¼ mile] west of Aubin. It is the result of the spontaneous combustion of a single seam of coal, which commenced twenty years ago, and has lost nothing of its activity. This seam is several yards thick, and runs east and west, as the ridge of the hill does. Its dip is about 45° south, and consequently opposite to the slope of the hill. It is easy to distinguish the outcrops of this seam on the parts

Mine of Buégne.

* The aspect of the alum mine of Fontaines, the desolation and broken state of the ground, at first view suggest the idea of volcanic phenomena. But on a more attentive examination we perceive, that the earth has been deranged only by sinking in; that there is no fissure which has any resemblance to the mouth of a crater; that the scorification and vitrification have been effected on the spot; that the products of these two operations do not resemble lavas; that the vapours always very evidently contain bitumen, and never muriate of ammonia; that the salts formed are sulphates; that besides no detonation is ever heard, and the ground experiences no commotion that can be compared to an earthquake: in short, if we set aside the heat and light produced by the combustion of the coal, and the aqueous and acidosulphurous vapours emitted, nothing similar to volcanic phenomena ever takes place.

Difference between these and volcanic fires.

of

... hill uninjured. They run horizontally about a ... the way down the hill. The works had not been ...ried very deep before the fire, but they occupied a considerable length on the outcrop.

The space deranged and altered by the fire exhibits nearly an oval figure. The shorter axis does not exceed 70 met. [76 yards]; but the greater, which is horizontal, may be 150 met. [164 yards]. The surface cannot be estimated at more than 60 ares [148 acres]. The whole of it has ceased to form a continued plane with the slope of the mountain, which is about 40°; and exhibits a depression pretty exactly resembling in figure the stern of a boat. Part of this surface is covered with solid aluminous incrustations, which resist the action of the rain in some degree, or are reproduced immediately after. It must be a rich mine, though not at present worked.

Mine of Bourlhones.

The mine of Bourlhones is the least of the four. It is half way up the hill that faces the mine of Buégne, and consequently in the same valley. Their distance from each other in a straight line is about 500 met. [546 yards.]

The fire that formed it has not continued above ten years. It is fed at the expense of a single seam of coal several yards thick, and inclining 30° or 40° east, consequently opposite to the slope of the hill.

The coal had not been worked to any extent, when it took fire. The combustion has not yet reached its highest degree of activity. The surface of the ground is partly covered with grass, partly sunk down, cracked, and torrefied. Copious vapours of water, sulphur, and bitumen, issue from it. Its shape is nearly circular, and it may be estimated at 30 ares [74 acres]. The aluminous salts are very abundant, but only in certain places; though by proper management their formation may be accelerated in others. No attempt has yet been made to work it.

Produce of the mines.

From the two mines, that are worked by two companies of adventurers, near 17000 myriagr. [167 tuns] of alum were made in 1809, which sold for about 120000 fr. [£ 5000].

VII. *The*

VIII.

The Croonian Lecture, on some Physiological Researches, respecting the Influence of the Brain on the Action of the Heart, and on the Generation of animal Heat. By Mr. B. C. Brodie, F. R. S. *

Influence of the brain on the action of the heart.

HAVING had the honour of being appointed, by the president of the Royal Society, to give the Croonian lecture, I trust, that the following facts and observations will be considered as tending sufficiently to promote the objects, for which the lecture was instituted. They appear to throw some light on the mode, in which the influence of the brain is necessary to the continuance of the action of the heart; and on the effect which the changes produced on the blood in respiration have on the heat of the animal body.

Not directly necessary to it.

In making experiments on animals to ascertain how far the influence of the brain is necessary to the action of the heart, I found, that, when an animal was pithed by dividing the spinal marrow on the upper part of the neck, respiration was immediately destroyed, but the heart still continued to contract circulating dark coloured blood; and that in some instances from ten to fifteen minutes elapsed, before its action had entirely ceased. I farther found, that, when the head was removed, the divided blood vessels being secured by a ligature, the circulation still continued, apparently unaffected by the entire separation of the brain. These experiments confirmed the observation of Mr. Cruickshank † and Mr. Bichat ‡, that the brain is not directly necessary to the action of the heart; and that, when the functions of the brain are destroyed, the circulation ceases only in consequence of the suspension of the respiration. This led me to conclude, that, if respiration was produced artificially, the heart would continue to contract for a still longer period of time after the removal of the brain.

* Philos. Trans. for 1811, p. 36.

† Philosophical Transactions 1795.

‡ Recherches Physiologiques sur la Vie et la Mort.

The

The truth of this conclusion was ascertained by the following experiment.

Exp. 1. On a rabbit. Communication cut off, and respiration continued artificially.

Exp. 1. I divided the spinal marrow of a rabbit in the space between the occiput and atlas, and having made an opening into the trachea, fitted into it a tube of elastic gum, to which was connected a small pair of bellows, so constructed, that the lungs might be inflated, and then allowed to empty themselves. By repeating this process once in five seconds, the lungs being each time fully inflated with fresh atmospheric air, an artificial respiration was kept up. I then secured the blood vessels in the neck, and removed the head, by cutting through the soft parts above the ligature, and separating the occiput from the atlas. The heart continued to contract, apparently with as much strength and frequency as in a living animal. I examined the blood in the different sets of vessels, and found it dark coloured in the venæ cavæ and pulmonary artery, and of the usual florid red colour in the pulmonary veins and aorta. At the end of twenty-five minutes from the time of the spinal marrow being divided, the action of the heart became fainter, and the experiment was put an end to.

No urine secreted.

With a view to promote the inquiry instituted by the society for promoting the knowledge of animal chemistry respecting the influence of the nerves on the secretions *, I endeavoured to ascertain, whether they continued after the influence of the brain was removed. In the commencement of the experiment I emptied the bladder of its contents by pressure; at the end of the experiment the bladder continued empty.

This experiment led me to conclude, that the action of the heart might be made to continue after the brain was removed, by means of artificial respiration, but that under these circumstances the secretion of urine did not take place. It appeared, however, desirable to repeat the experiment on a larger and less delicate animal; and that, in doing so, it would be right to ascertain whether under these circumstances the animal heat was kept up to the natural standard.

* Philosophical Transactions for 1809. Journal vol. xxvi, p. 136.

Exp.

Exp. 2. I repeated the experiment on a middle sized dog. The temperature of the room was 63° of Fahrenheit's thermometer. By having previously secured the carotid and vertebral arteries, I was enabled to remove the head with little or no hæmorrhage. The artificial respirations were made about twenty-four times in a minute. The heart acted with regularity and strength. Exp. 2. On a dog.

At the end of 30 minutes from the time of the spinal marrow being divided, the heart was felt through the ribs contracting 76 times in a minute. Action of the heart.

At 35 minutes the pulse had risen to 84 in a minute.

At one hour and 30 minutes the pulse had risen to 88 in a minute.

At the end of two hours it had fallen to 70, and at the end of two hours and a half to 35 in a minute, and the artificial respiration was no longer continued.

By means of a small thermometer with an exposed bulb, I measured the animal heat at different periods. Animal heat.

At the end of an hour the thermometer in the rectum had fallen from 100° to 94°.

At the end of two hours a small opening being made in the parietes of the thorax, and the ball of the thermometer placed in contact with the heart, the mercury fell to 86°, and half an hour afterward in the same situation it fell to 78°.

In the beginning of the experiment I made an opening into the abdomen; and, having passed a ligature round each ureter about two inches below the kidney, brought the edges of the wound in the abdomen together by means of sutures. At the end of the experiment no urine was collected in the ureters above the ligatures. No urine secreted.

On examining the blood in the different vessels, it was found of a florid red colour in the arteries, and of a dark colour in the veins, as under ordinary circumstances. Blood.

During the first hour and a half of the experiment there were constant and powerful contractions of the muscles of the trunk and extremities, so that the body of the animal was moved in a very remarkable manner, on the table, on which it lay, and twice there was a copious evacuation of fæces. Muscular contractions.

Exp.

Exp. 3. The experiment was repeated on a rabbit. The temperature of the room was 60°. The respirations were made from 30 to 35 in a minute. The actions of the heart at first were strong and frequent: but at the end of one hour 40 minutes the pulse had fallen to 24 in a minute.

Blood.

The blood in the arteries was seen of a florid red, and that in the veins of a dark colour.

A small opening was made in the abdominal muscles, through which the thermometer was introduced into the abdomen, and allowed to remain among the viscera.

Animal heat.

At the end of an hour the heat in the abdomen had fallen from 100° to 89°. At the end of an hour and forty minutes in the same situation the heat had fallen to 85°, and when the bulb of the thermometer was placed in the thorax in contact with the lungs the mercury fell to 82°.

Seemingly not dependent on chemical changes of the blood in respiration.

It has been a very generally received opinion, that the heat of warm blooded animals is dependent on the chemical changes produced on the blood by the air in respiration. In the two last experiments the animals cooled very rapidly, notwithstanding the blood appeared to undergo the usual changes in the lungs; and I was therefore induced to doubt whether the above mentioned opinion respecting the source of animal heat is correct. No positive conclusions however could be deduced from these experiments. If animal heat depends on the changes produced on the blood by the air in respiration, its being kept up to the natural standard, or otherwise, must depend on the quantity of air inspired, and on the quantity of blood passing through the lungs in a given space of time: in other words, it must be in proportion to the fulness and frequency of the pulse, and the fulness and frequency of the inspirations. It therefore became necessary to pay particular attention to these circumstances.

Exp. 4. On a small dog.

Exp. 4. The experiment was repeated on a dog of a small size, whose pulse was from 130 to 140 in a minute, and whose respirations, as far as I could judge, were performed from 30 to 35 times in a minute.

The temperature of the room was 63°. The heat in the rectum of the animal at the commencement of the experiment

ment was 99°. The artificial inspirations were made to correspond as nearly as possible to the natural inspirations both in fulness and frequency.

At 20 minutes from the time of the dog being pithed, the heart acted 140 times in a minute with as much strength and regularity as before: the heat in the rectum had fallen to 96½.

At 40 minutes the pulse was still 140 in a minute: the heat in the rectum 92½.

At 55 minutes the pulse was 112, and the heat in the rectum 90°.

At one hour and 10 minutes the pulse beat ninety in a minute, and the heat in the rectum was 88°.

At one hour and 25 minutes the pulse had sunk to 30, and the heat in the rectum was 85°. The bulb of the thermometer being placed in the bag of the pericardium, the mercury stood at 85°, but among the viscera of the abdomen it rose to 87½.

During the experiment there were frequent and violent contractions of the voluntary muscles, and an hour after the experiment was begun, there was an evacuation of fæces.

Exp. 5. The experiment was repeated on a rabbit, whose respirations, as far as I could judge, were from 30 to 40 in a minute, and whose pulse varied from 130 to 140 in a minute. The temperature of the room was 57°. The heat in the rectum, at the commencement of the experiment, was 101½. The artificial respirations were made to resemble the natural respirations as much as possible, both in fulness and frequency. Exp. 5. On a rabbit.

At 15 minutes from the time of the spinal marrow being divided, the heat in the rectum had fallen to 98½°.

At the end of half an hour the heart was felt through the ribs, acting strongly 140 times in a minute.

At 45 minutes the pulse was still 140; the heat in the rectum was 94°.

At the end of an hour the pulse continued 140 in a minute; the heat in the rectum was 92°; among the viscera of the abdomen 94°; in the thorax, between the lungs and pericardium, 92°.

During

During the experiment, the blood in the femoral artery was seen to be of a bright florid colour, and that in the femoral vein of a dark colour, as usual.

The rabbit voided urine at the commencement of the experiment; at the end of the experiment no urine was found in the bladder.

Exp. 6. On a large rabbit.

Exp. 6. I procured two rabbits of the same colour, but one of them was about one fifth smaller than the other. I divided the spinal marrow of the larger rabbit between the occiput and atlas. Having secured the vessels in the neck, and removed the head, I kept up the circulation by means of artificial respiration as in the former experiments. The respirations were made as nearly as possible similar to natural respirations.

In 23 minutes after the spinal marrow was divided, the pulse was strong, and 130 in a minute: the ball of the thermometer being placed among the viscera of the abdomen, the mercury stood at 96°.

At 34 minutes the pulse was 120 in a minute: the heat in the abdomen was 95°.

At the end of an hour the pulse could not be felt, but on opening the thorax the heart was found acting, but slowly and feebly. The heat in the abdomen was 91°; and between the lobes of the right lung 88°.

During the experiment, the blood in the arteries and veins was seen to have its usual colour.

Comparative experiment on a smaller.

In this therefore, as in the preceding experiments, the heat of the animal sunk rapidly, notwithstanding the continuance of the respiration. In order to ascertain whether any heat at all was generated by this process, I made the following comparative experiment. The temperature of the room being the same, I killed the smaller rabbit by dividing the spinal marrow between the occiput and atlas. In consequence of the difference of size, *cæteris paribus*, the heat in this rabbit ought to diminish more rapidly than in the other; and I therefore examined its temperature at the end of 52 minutes, considering that this would be at least equivalent to examining that of the larger rabbit at the end of an hour. At 52 minutes from the time of the smaller rabbit being killed, the heat among the viscera of the abdomen

domen was 92°, and between the lobes of the right lung it was 91°. From this experiment, therefore, it appeared, not only that no heat was generated in the rabbit, in which the circulation was maintained by artificial respiration, but that it even cooled more rapidly than the dead rabbit.

No heat generated by artificial respiration.

At the suggestion of professor Davy, who took an interest in the inquiry, I repeated the foregoing experiment on two animals, taking pains to procure them more nearly of the same size and colour.

Exp. 7. I procured two large full grown rabbits of the same colour, and so nearly equal in size, that no difference could be detected by the eye.

Exp. 7. Two large rabbits of equal size: in one artificial respiration kept up, in the other not.

The temperature of the room was 57°, and the heat in the rectum of each rabbit previous to the experiment was 100½.

I divided the spinal marrow in one of them, produced artificial respiration, and removed the head after having secured the vessels in the neck. The artificial respirations were made about 35 times in a minute.

During the first hour, the heart contracted 144 times in a minute.

At the end of an hour and a quarter the pulse had fallen to 136 in a minute, and it continued the same at the end of an hour and a half. At the end of an hour and forty minutes the pulse had fallen to 90° in a minute, and the artificial respiration was not continued after this period.

Half an hour after the spinal marrow was divided, the heat in the rectum had fallen to 97°.

At 45 minutes the heat was 95½.

At the end of an hour the heat in the rectum was 94°.

At an hour and a quarter it was 92°.

At an hour and a half it was 91°.

At an hour and forty minutes, the heat in the rectum was 90½, and in the thorax, within the bag of the pericardium, the heat was 87½.

The temperature of the room being the same, the second rabbit was killed by dividing the spinal marrow, and the temperature was examined at corresponding periods.

Half an hour after the rabbit was killed, the heat in the rectum was 99°.

At

At 45 minutes it had fallen to 98°.

At the end of an hour the heat in the rectum was 96½.

At an hour and a quarter it was 95°.

At an hour and a half it was 94°.

At an hour and forty minutes the heat in the rectum was 93°, and in the bag of the pericardium 90½.

The following table will shew the comparative temperature of the two animals at corresponding periods.

Table of their comparative temperatures.

Time.	Rabbit with artificial respiration.		Dead Rabbit.	
	Therm. in the Rectum.	Therm. in the Pericardium.	Therm. in the Rectum.	Therm. in the Pericardium.
Before the experiment.	100½		100½	
30 min. aft.	97		99	
45 ——	95½		98	
60 ——	94		96½	
75 ——	92		95	
90 ——	91		94	
100 ——	90½	87½	93	90½

The production of animal heat seems not to depend on respiration.

In this experiment, the thorax, even in the dead animal, cooled more rapidly than the abdomen. This is to be explained by the difference in the bulk of these two parts. The rabbit in which the circulation was maintained by artificial respiration cooled more rapidly than the dead rabbit: but the difference was more perceptible in the thorax than in the rectum. This is what might be expected, if the production of animal heat does not depend on respiration; since the cold air, by which the lungs were inflated, must necessarily have abstracted a certain quantity of heat, particularly as its influence was communicated to all parts of the body, in consequence of the continuance of respiration.

Objection.

It was suggested that some animal heat might have been generated, though so small in quantity as not to counterbalance the cooling powers of the air thrown into the lungs. It is difficult, or impossible, to ascertain with perfect accuracy, what effect cold air thrown into the lungs would have

have on the temperature of an animal under the circumstances of the last experiment, independently of any chemical action on the blood: since, if no chemical changes were produced, the circulation could not be maintained, and if the circulation ceased, the cooling properties of the air must be more confined to the thorax, and not communicated in an equal degree to the more distant parts. The following experiment, however, was instituted as likely to afford a nearer approximation to the truth, than any other that could be devised.

Exp. 8. I procured two rabbits of the same size and colour: the temperature of the room was 64°. I killed one of them by dividing the spinal marrow, and immediately, having made an opening into the left side of the thorax, I tied a ligature round the base of the heart, so as to stop the circulation. The wound in the skin was closed by a suture. An opening was then made into the trachea, and the apparatus for artificial respiration being fitted into it, the lungs were inflated, and then allowed to collapse as in the former experiment, about 36 times in a minute. This was continued for an hour and a half, and the temperature was examined at different periods. The temperature of the room being the same, I killed the second rabbit in the same manner, and measured the temperature at corresponding periods. The comparative temperature of the two dead animals, under these circumstances, will be seen in the following table.

Exp. 8. Attempt to remove this.

Time.	Dead Rabbit whose lungs were inflated.		Dead Rabbit whose lungs were not inflated.	
	Therm. in the Rectum.	Therm. in the Thorax.	Therm. in the Rectum.	Therm. in the Thorax.
Before the experiment.	100		100	
30 min. aft.	97		98	
45 ———	95½		96	
60 ———	94		94½	
75 ———	92½		93	
90 ———	91	86	91½	88½

Tabulated results.

In

No animal heat apparently produced by respiration.

In this last experiment, as may be seen from the above table, the difference in the temperature of the two rabbits, at the end of an hour and a half in the rectum, was half a degree, and in the thorax two degrees and a half; whereas, in the preceding experiment, at the end of an hour and forty minutes, the difference in the rectum was $2\frac{1}{2}$ degrees, and in the thorax 3 degrees. It appears, therefore, that the rabbit in which the circulation was maintained by artificial respiration cooled more rapidly on the whole, than the rabbit whose lungs were inflated in the same manner after the circulation had ceased. This is what might be expected if no heat was produced by the chemical action of the air on the blood; since in the last case the cold air was always applied to the same surface, but in the former it was applied always to fresh portions of blood, by which its cooling powers were communicated to the more distant parts of the body.

In the course of the experiments which I have related, I was much indebted to several members of the Society for promoting the Knowledge of Animal Chemistry, for many important suggestions, which have assisted me in prosecuting the inquiry. Mr. Home, at my request, was present at the seventh experiment. Dr. E. N. Bancroft was present at, and assisted me in the second experiment: and Mr. William Brande lent me his assistance in the greater part of those which were made. I have been farther assisted in making the experiments by Mr. Broughton, surgeon of the Dorsetshire regiment of militia, and Mr. Richard Rawlins, and Mr Robert Gatcombe, students in surgery.

Many other experiments gave similar results.

I have selected the above from a great number of similar experiments, which it would be needless to detail. It is sufficient to state, that the general results were always the same; and that, whether the pulse was frequent or slow, full, or small, or whether the respirations were frequent or otherwise, there was no perceptible difference in the cooling of the animal.

General conclusions.

From the whole we may deduce the following conclusions:

1. The influence of the brain is not directly necessary to the action of the heart.

2. When

2. When the brain is injured or removed, the action of the heart ceases, only because respiration is under its influence, and if under these circumstances respiration is artificially produced, the circulation will still continue.

3. When the influence of the brain is cut off, the secretion of urine appears to cease, and no heat is generated; notwithstanding the functions of respiration and the circulation of the blood continue to be performed, and the usual changes in the appearance of the blood are produced in the lungs.

4. When the air respired is colder than the natural temperature of the animal, the effect of respiration is not to generate, but to diminish animal heat.

Addition to the Croonian Lecture for the Year 1810.
(*P.* 207.)

Artificial respiration produces similar changes on the blood with natural.

In the experiments above detailed, where the circulation was maintained by means of artificial respiration after the head was removed, I observed that the blood, in its passage through the lungs, was altered from a dark to a scarlet colour; and hence I was led to conclude, that the action of the air produced in it changes analogous to those, which occur under ordinary circumstances. I have lately, with the assistance of my friend Mr. W. Brande, made the following experiment, which appears to confirm the truth of this conclusion.

Experiment to show this.

An elastic gum bottle, having a tube and stop-cock connected with it, was filled with about a pint of oxigen gas. The spinal marrow was divided in the neck of a young rabbit, and, the blood vessels having been secured, the head was removed, and the circulation was maintained by inflating the lungs with atmospheric air for five minutes, at the end of which time the tube of the gum bottle was inserted into the trachea, and carefully secured by a ligature, so that no air might escape. By making pressure on the gum bottle, the gas was made to pass and repass into and from the lungs about thirty times in a minute. At first, the heart acted one hundred and twenty times in a minute, with regularity and strength; the thermometer, in the rectum, rose to 100°. At the end of an hour, the heart acted as frequently as before, but more feebly; the blood in

e arteries was very little more florid than that in the ; the thermometer in the rectum had fallen to 93°. gum bottle was then removed. On causing a stream of the as, which it contained, to pass through lime water, th resence of carbonic acid was indicated by the liquid b instantly rendered turbid. The proportion of car- acid was not accurately determined; but it appeared i rm about one half of the quantity of gas in the bottle.

B. C. BRODIE.

IX.

Notes by Mr. J. H. Hassenfratz on the Disoxidation of Oxide of Iron by Hidrogen Gas.*

Disoxidation of iron by hidrogen gas.

DESIROUS of repeating the experiment of Messrs. Priestley, Chaussier, and Amadeus Berthollet, on the disoxidation of iron by hidrogen gas, I last year employed Mr. Charbaut, then a pupil of the School of Mining, to make the experiment in my presence. He proceeded in two ways: in one the iron was disoxidated by hidrogen, in the other by oil and charcoal. In the latter experiment the metal was fused by increasing the temperature, so as to obtain a button of iron.

More weight lost than in the reduction by oil and charcoal.

On comparing these two modes, I was astonished to find, that the diminution of weight of the oxidule of iron by hidrogen was always greater than that effected by oil and charcoal. The perplexity into which I was thrown by these results induced me to repeat the experiment anew.

The experiments repeated.

Accordingly this year I employed at the Practical School of Mining the pupil Desroches, of whose sagacity and precision I was previously satisfied, to decompose by the action of hidrogen gas oxidules of iron from the valley of Aoste, and specimens of oligist iron from Elba, while other pupils assayed the same minerals before me in the dry way. The results obtained agreed precisely with those of last year.

* Ann. de Chim. vol. lxiii, p. 117.

Finally

Finally, at my departure from Moutiers, I requested the pupil Desroches to make fresh experiments on the decomposition of the oxidule of iron of Cogne, and oligist iron of Elba. The official statement of these experiments, certified by engineer Leboullenger, I shall proceed to lay before the public.

Experiments on the Disoxidation of Oxide and Oxidule of Iron.

Disoxidation of metals by heat.

It has been said, that all metals are capable of being disoxidated by heat, and that the temperature required for their reduction is much higher than that of their oxidation. It is easy to conceive, that, if the tendency to take the aeriform state be less powerful than the attraction of the oxigen by the metal, the oxigen will be solidified, and an oxide formed: but if the elasticity be superior to the attraction, no combination, or oxidation, will take place. This occurs in the manufacture of minium: too strong a fire produces massicot, and sometimes reduces the oxide entirely. It is observable too, that, beyond a certain temperature, the time required for oxidation is in the inverse ratio of the heat. This I had an opportuity of observing in the oxidation of iron by heat last year. Having taken some pure filings of good iron, and exposed them to a graduated heat, I obtained in a very little time an addition of 32 per cent: I increased the heat and the current of air, but it was a long while before I gained 40 per cent: and it was not without a great deal of trouble, and a very long time, that I obtained the known result of 45 per cent, which I could not exceed.

Oxidation by heat.

Are all metals reducible by heat?

But is heat alone capable of reducing all metals? This question is already decided with respect to some, which have but a feeble attraction for oxigen. As to those which retain it forcibly, it may be, that the heat requisite for their disoxidation is superior, or at least equal to that necessary for their fusion; and then it would be impossible to separate the gas from the metal.

But if a powerful disoxidizer be employed in conjunction with caloric, so great a heat will not be required to reduce the metal: this no doubt induced the younger Mr. Ber-

 thollet

thollet to employ hidrogen gas in his experiments, which I repeated as follows.

Two specimens of native oxide of iron exposed to the action of hidrogen gas.

I took 5 gram. [77·23 grs.] of oxidulated iron of Cogne, and a similar quantity of oligist iron of Elba, and placed them in a semicircular tube with two compartments, intended each to hold one of the oxides. This tube, furnished with a long stem curved at one end, was placed in a gunbarrel open at both ends, previously cleaned, and coated externally with loam, to preserve it from oxidation. At the curved end of the stem, which answered to one of the ends of the gunbarrel, a curved tube was luted, terminating under water, and intended to afford a passage to the superfluous hidrogen gas and the vapours of the apparatus, which were collected in bottles filled with water, and resting on a perforated test, underneath which the tube opened. The gunbarrel was placed 4 inches from the grate in a furnace, the opening of which was 8 in. [8·5 Eng.] wide, and 12 [12·8] high from the grate, which rested immediately on the nozzle of a pair of forge bellows. To the other end of the gunbarrel was fitted a tube, curved likewise, communicating with a cock placed under a jar completely immersed in a tub of water, the pressure of which was intended to force out the hidrogen gas, with which the jar was kept constantly filled.

All the parts of the apparatus being securely fixed and luted, it was found to be air tight, by passing a measured portion of air from the jar into the receivers at the other extremity.

Hidrogen gas was then prepared from iron filings and diluted sulphuric acid; the furnace was filled with charcoal; the fire was kindled, and blown gently. When the gunbarrel was redhot, which might easily be seen through the glass tubes at its two extremities, the cock was closed, and the jar filled with hidrogen gas. This gas was then passed through the apparatus, by opening the cock a little. Part of the gas was absorbed; and the remainder, which was received in the bottles with the aqueous vapour that condensed in them, was returned into the jar. In this process the oxidule and oligist iron at this temperature, presenting to the gas a porous mass, which it could easily traverse, each particle

particle was surrounded with hidrogen, gave up its oxigen, and formed vapour of water, which was perceived to condense in the curved tube at the extremity of the barrel; and which, at the close of the operation, when the heat was excessive, traversed all the water in the tube and the bottles, producing wreaths of white vapour, similar to those issuing from rockets.

Care was taken to keep in the tub a sufficient quantity of water to cover the jar; and also such a quantity of gas in the jar, that the pressure should be always nearly the same, and the passage of the gas consequently uniform. The fire was gradually increased; but absorption still taking place, it was stopped when it appeared to be at a maximum. I then thought I observed, that the fire was not stronger than might have been produced in a common furnace, simply supplied with the current of air passing through the ash-hole, so that the bellows were useless. This however I mention only as a conjecture, more decisive proofs being necessary to ascertain it.

We were employed in the fatiguing operations of supplying fuel, filling the jar with hidrogen, emptying under it the bottles containing the hidrogen that had passed through the apparatus, preparing others to receive that which was constantly issuing from it, and keeping up the level of the water in the two tubs, for four hours and a half. At the expiration of this time the iron oxides having absorbed the eight bottles of hidrogen gas that had been prepared, it was necessary to put an end to the experiment: and for my own satisfaction, I dilated the end of the gunbarrel that contained the plate iron stem of the tube, and the curved end of this stem enabled me to draw out the tube with an iron wire. I weighed the iron immediately: that of Cogne weighed 4·19 gr. [64·72 grs.]; that of Elba, 3·77 gr. [58·23 grs.] Their weight after the experiment,

The oxidule of Cogne had become altogether stony, and of a yellowish gray. Many pieces of the oligist iron had lost their metallic lustre, having turned yellowish, and acquired a duller lustre like that of silver: but I was not certain, that this iron was reduced, since no superfluous hidrogen gas had passed over, which was perhaps incomplete,

and therefore repeated.

This induced me to continue the experiment. The apparatus was fitted up again as before; and, after I had made a considerable quantity of hidrogen gas, and taken the precautions abovementioned, the fire was kindled, and gas was passed over, till no sensible absorption took place. All the fuel remaining in the furnace was then consumed, by continuing the action of the bellows; and during this combustion gas was still passed over, that no water might introduce itself into the gunbarrel during its cooling, which was thus effected very gradually. The jar was cooled full of gas, and the apparatus taken to pieces as before.

Weights.

The oxidule of Cogne now weighed 3·69 gr. [56·99 grs.] and the oligist iron of Elba 3·32 gr. [51·28 grs.]

State of the iron of Cogne,

The oxidulated iron of Cogne had altogether lost its metallic lustre: its yellowish aspect exhibited spots separable from the yellowish gray ground, which, examined with a lens, exhibited a sort of metallic arborizations of the colour of cast iron. On hammering it acquired lustre, and flattened, but at length broke (owing, no doubt, to the impurities of the ore). Its fracture was then very brilliant, and resembling that of iron.

and of that of Elba.

The iron of Elba had likewise lost its metallic lustre, but had assumed a duller, resembling that of silver. Some parts had the appearance of a sponge, coloured superficially with a fugitive tint, varying from yellow to that of coarse Prussian blue, and thence to violet. All its species were malleable, and were reduced thinner under the hammer than the iron of Cogne before they broke. After the experiment the specimens were analysed, to determine exactly the quantity of iron they contained.

The iron of Cogne analysed.

The 3·69 gram. [56·99 grs.] of iron of Cogne were treated with nitromuriatic acid. A large quantity of nitrous acid was evolved in red fumes, which proved the great disoxidation of the oxidule. That nothing might be lost, it was not levigated; which did not prevent the action from being brisk, and completed in a few hours, even without heating. This was necessarily the case; for, the iron having been rendered very porous by the process of disoxidation, every particle of the metal was separated, as it were from the rest, and from the earthy particles, so that the acid

acid could act on them with facility. Having evaporated to dryness, water was added, and a little muriatic acid, to take up the oxide of iron separated by drying. A whitish granular precipitate was obtained; which, collected on a filter, washed and calcined, became very white, and weighed 0·36 of a gram. [5·56 grs.] This was silex. The iron of Cogne analysed.

The solution, of a fine orange yellow colour, was saturated by ammonia; only taking care to leave a slight excess of acid, to hold in solution all the earths, that might have fallen down with the oxide of iron. This oxide was collected on a filter; and the liquor assayed by carbonate and oxalate of ammonia to detect the presence of alumine and lime. No precipitate being thrown down, the liquor was evaporated to dryness; and the muriates, oxalates, and carbonates of ammonia and magnesia (for, if there were any earth present, it could only be magnesia) were afterward calcined. The ammoniacal salts were volatilized; and a substance was left (it was an oxalate), which, having been again calcined on a porcelain test, became white, and weighed 0·31 of a gr. [4·79 grs.] It was magnesia.

As the oxide of iron remaining on the filter might still contain other metals and earths, it was treated by acetic acid, and heated to dryness. Water was then added, and it was heated to dryness again. Lastly, after having added more water, cleaned the capsule, and heated a little; the solution was filtered, evaporated to dryness, and the residuum calcined on a porcelain test. The whole was volatilized, except a blackish, alkaline substance, incapable of being weighed, which was presumed to be lime (proceeding from the filtres) contaminated by the carbon of the decomposed acetic acid.

The iron left on the filtre was treated with muriatic acid, because it was suspected to contain silex; for the nitromuriatic acid might have dissolved a portion of this earth in its state of disintegration, and the ammonia would have precipitated the silex with the iron. This in fact was the case: for, after having filtered the solution of iron, there was a residuum, which, when washed and calcined, became very white, and weighed 0·2 of a gram. [3·09 grs.]; and this was silex.

The

The iron was precipitated by ammonia, which was boiled on it repeatedly to remove the acid; and, after calcination in the open air, 4·08 gr. [63·02 grs.] of fine red oxide of iron were obtained.

Results.

Thus the oxidulated iron of Cogne yielded

Red oxide of iron	-	4·07 * grammes	= 62·86 grains
Silex	- - -	0·56	= 8·65
Magnesia	- -	0·31	= 4·79

Accordingly it contained 0·87 of a gr. [13·44 grs.] of earth: and consequently of the 5 gr. [77·23 grs.] employed only 4·13 gr. [63·75 grs.] were oxidule. Now in the experiment of the disoxidation the 5 gr. [77·23 grs.] were reduced to 3·69 [56·99 grs.]: 4·13 gr. [63·75 grs.] of oxidule therefore contained 1·31 gr. [20·23 grs.] of oxigen (lost in the experiment;) and consequently the oxidulated iron of Cogne is at $\frac{13100}{413}$ per cent, or 31·72 per cent.

Results of the analysis of the iron of Elba.

In like manner the oligist iron of the isle of Elba yielded by analysis

Red oxide of iron	-	4·4 grammes	= 67·96 grains
Silex	- - -	0·25	= 3·86.

Thus, as there were 0·25 of a gr. [3·86 grs.] of earth, there were only 4·75 gr. [73·37 grs.] of oxide in the substance employed: and, as the 5 gr. [77·23 grs.] were reduced in the experiment to 3·32 gr. [51·28 grs.], they had lost 1·68 gr. [25·95 grs.]; consequently there were 1·68 gr. [25·95 grs.] of oxigen to 4·75 gr. [73·37 grs.] of oxide. The oligist iron of the isle of Elba therefore has $\frac{16800}{475}$ per cent of oxigen, or 35·37 per cent nearly.

If we may be allowed to depend on these results, we may conclude, that the oxidulated iron of Cogne contains 32 of oxigen in 100 of the oxidule; and that the oligist iron of Elba contains 35 of oxigen in 100 of oxide.

Other Results.

Results from the iron of Cogne,

It has been seen, that there were 4·13 gr. [63·75 grs.] of oxidule in the iron of Cogne, and that this iron was oxidized in the proportion of 31·72 per cent. It has appeared too, that the 3·69 gr. [56·99 grs.] of iron of Cogne

* Just above it is said 4·08 gram. C.

obtained

obtained by disoxidation contained 0·87 [13·44 grs.] of earth; consequently there were $3·69 - 0·87 = 2·82$ gr. [43·56 grs.] of pure iron. In the analysis of this iron 4·07 gr. [62·86 grs.] of red oxide were obtained: the red oxide therefore contained $4·07 - 2·82 = 1·25$ gr. [19·31 grs.] of oxigen; and consequently was at $\frac{12500}{282}$ per cent, or 44 per cent and upward, (allowing for any trifling errour).

As to the iron of Elba, we find by calculation, that the red oxide obtained was at 43 per cent and upward, allowing likewise for any trifling errour; and if we take the mean of the two results, admitting decimals and allowing for any little errour, we shall find, that the red oxide is at 44 per cent. and that of Elba.

In some troublesome experiments, which I shall not describe, I was employed to obtain hidrogen by the decomposition of water. For this purpose I took some very fine iron wire, which I weighed and introduced into a gunbarrel, adapted to this a retort filled with water, and proceeded in the usual way. After the process I had a wire extremely increased in size, consisting of an assemblage of octaedral crystals so small as to be visible only by a lens, and forming a fragile wire oxided in all parts. I weighed it, and as there were still some parts that had been less heated, and not perfectly oxided, I pulverised the oxidule, subtracted the iron thus separated, and on calculation found I had an oxidule of 32 per cent and upward.

Hidrogen obtained by passing water over iron wire.

State of the iron.

DESROCHES.

This is to certify, that these experiments were made at the laboratory of the School of Mines in the month of August, 1809.

LE BOULLENGER.

Observations by Mr. HASSENFRATZ.

It follows from the experiments of Mr. Desroches, that the oxidule of Cogne lost 0·317 of oxigen, which amounts to 46 parts to 100 of iron; and that the oligist iron of Elba lost 0·3537, which would make more than 54 to 100 of iron.

Observations by Mr. Hassenfratz.

The

[T]he oxidule of Cogne, treated with charcoal, in one [expe]riment yielded from 5 gr. [77·23 grs.) a button con[taini]ng 3·42 gr. [52·82 grs.] of iron, and 0·66 of a gr. [10·19 grs.] of scoriæ, which would make the loss about 27 to 100 of iron; and in another experiment the 5 gr. yielded a button containing 3·38 gr. [52·21 grs.] of iron, and 0·78 [12·05 grs.] of scoriæ, making the loss 25 to 100 of iron. We will take the highest term, 27.

The oligist iron of Elba yielded from 5 gram. a button of iron weighing 3.6 [55·6 grs.] and 0·1 [1·54 gr.] of scoriæ; which would make the loss 30 to 100 of iron.

More loss in the reduction by hidrogen than in that by charcoal.

Thus the difference of loss in the two modes of reducing the oxide of iron would be for the oxidule of iron of Cogne 46 by hidrogen, and 27 by charcoal; and for the oligist iron of Elba 54 by hidrogen and 30 by charcoal.

Possible causes of the difference.

With regard to the causes, that may produce this difference, we may distinguish three: 1, the charcoal, that combines with the iron, when the metal is fused with this combustible: 2, the oxigen, that may remain combined with the iron in the metallic button obtained: 3, the action of the hidrogen on the iron, the gas dissolving and carrying off some of the metal.

Addition to the iron by carbon,

Desirous of knowing what might be the influence of each of these causes, I fused in a crucible lined with charcoal 5 gr. of iron wire previously soaked in oil, and obtained a button weighing 5·13. Hence it follows, that somewhat less than 0·03 of carbon was combined with it.

and by carbon and oxigen.

I afterward dissolved 5 gr. of iron in nitric acid, in order to oxidate the metal to a maximum; moistened the oxide with oil; placed it in a crucible lined with charcoal to fuse it; and obtained a button weighing 5·2: consequently 0·04 of carbon and oxigen had combined with the iron.

Supposing, that 0·01 of carbon and oxigen remained in the buttons obtained from the oxidule of Cogne and the oligist iron of Elba, it would follow, that the oxidule of Cogne had lost near 32 per cent of oxigen, and the iron of Elba near 36.

Difference between the two ores.

These two results agree in placing the oxidule of Cogne in the rank of black oxides obtained by the decomposition of water over iron; for this proportion of 32 is nearly what

what I have deduced from the experiments of several able chemists on the composition and decomposition of oxidules of iron. It is also the same as Mr. Desroches has deduced from the experiments he made this year at Moutiers.

It follows too from these experiments, that the oligist iron is more oxided than the oxidule, as the learned Mr. Haüy had concluded from the colour of these two ores when powdered.

Loss in the reduction by hidrogen not thus accounted for.

But when we have taken account of the carbon and oxigen combined in the metallic button obtained from the disoxidation of oxides of iron by charcoal, it appears, that the loss they undergo in their reduction is still less than that which occurs when they are disoxided by hidrogen; since in the latter case the oxidule of Cogne lost 46 to 100 of iron, while it lost but 32 in the reduction by charcoal; and the iron of Elba lost 54 with hidrogen, and only 36 with charcoal.

Iron apparently dissolved and carried off by the hidrogen.

Is this difference ascribable to the solvent action of hidrogen? Some observations seem to warrant this conclusion. 1, When the hidrogen gas obtained by the decomposition of water passed over iron, or by dissolving this metal in acids, or otherwise, is preserved in jars over water, the interior of the jars sometimes becomes coated with a slight stratum of oxide of iron. 2, At the end of the account of his experiments Mr. Desroches had added the following note. "A great deal of ferruginous hidrogen gas was evolved, as I found by its smell; so that probably some iron was lost in the passage of the hidrogen gas through it."

This deserves inquiry.

I do not think however, as Mr. Desroches observes, that we should hastily conclude hidrogen to have a solvent action on iron from his experiments alone. They should be repeated and varied in several ways, before we decide on a fact of such importance. It is sufficient for me at present to have called the attention of chemists to a result, that is worthy their consideration.

X.

Determination of the Quantity of Hidrogen and of Ammonia contained in the Amalgam of Ammonia: by Messrs. GAY-LUSSAC AND THENARD*.

Quantity of hidrogen contained in amalgam of ammonia.

WE took 3·069 gr. [47·403 grs.] of mercury, placed them in a small cupel of sal ammoniac at the negative pole, and, when their bulk was about quintupled, threw them into a conical glass filled with water, in which was previously placed a small jar also filled. The bubbles of air, that might have been adherent to the button of amalgam, were at first suffered to escape, by keeping the jar close to the sides of the glass; after which the jar was raised, so as to let the button fall to the bottom, and all the hidrogen gas arising from it was collected gradually in the upper part of the jar. Six buttons of amalgam, each made with a similar quantity of mercury, and treated in this manner successively, produced such a quantity of hidrogen, that the mercury had absorbed 3·47 times its bulk of this gas in passing to the state of soft amalgam. To avoid every source of errour, the bulk of the mercury employed and of the hidrogen collected was measured in the same tube, which was accurately graduated.

A second experiment, made also with six buttons of soft amalgam, having afforded results scarcely differing from the preceding, they may be considered as exact, or at least as approaching very nearly to the truth. It may happen however, that, on a repetition of these experiments, other numbers than ours may be found; and this must necessarily be the case, if the amalgam were not made so as to obtain it soft, or so that the mercury entering into it should have its bulk at least quintupled.

Quantity of ammonia contained in amalgam of ammonia.

We imagined at first, that by amalgamating a given quantity of mercury and deducting the known weight of the mercury and the hidrogen it contained, we should find exactly the quantity of ammonia entering into the amalgam. But we soon

* Annal. de Chim. vol. lxxiii, p. 209. Extracted from a paper read to the Institute, September, 1809.

found

found, that this mode of analysis was very inaccurate: 1st, because the amalgam is half destroyed before it is well dried: 2dly, because this amalgam displaces a volume of air, of which it is difficult to take account: 3dly, and lastly, because, on introducing it into the phial, the hidrogen and ammoniacal gas evolved take the place of a quantity of air, which cannot be estimated, and must necessarily occasion great errours in the results. Hence the weights of all differed from one another. One gave us on 3·069 gr. of mercury an augmentation of 0·002; another, an increase of 0·003; a third, of 0·0045; and a fourth, of 0·001 only. It is even possible, that a loss of weight might appear, since the air of the phial is replaced by hidrogen and ammoniacal gas. Such no doubt were the causes of Mr. Davy's mistake, when he found that mercury, in forming an amalgam, was increased only a twelve-thousandth of its weight.

Mode of analysis employed.

Impelled by these reasons to reject this mode of analysis, we employed the following, which we consider as very exact. Knowing the quantity of hidrogen contained in the ammoniacal amalgam; and not doubting, that the hidrogen and ammonia were in a uniform proportion to each other in this amalgam, we had recourse to this proportion, to determine the whole quantity of the ammonia it contained. For this purpose we converted into amalgam 3·069 gr. [47·403 grs.] of mercury; after the amalgam was well dried with blotting paper, we introduced it immediately into a small jar very dry, and a quarter filled with mercury; and immediately too clapping a finger on the mouth of the jar, we shook the whole together for a few minutes. In this way the portion of amalgam that still subsisted was decomposed, the hidrogen and ammonia it contained returning to the state of gas; for the moment the little jar was immersed in mercury and unstopped, the mercury was seen to sink. Three other similar experiments were made, in order to obtain more decisive results; and after each experiment the gasses were passed into one and the same very dry tube filled with mercury. When they were thus all collected in the tube, the quantity of ammonia they contained was determined by agitating them with water. Then, to

iow exactly the quantity of hidrogen present, which, s residuum, was mingled with a great deal of common , it was burnt in Volta's eudiometer, with an addition of drogen and oxigen in known quantities, in order to r the combustion complete and more easy.

Results. us we found, that in these gasses the ammonia was to idrogen as 28 to 23. But as we knew, that the mer- absorbed 3·47 times its bulk of hidrogen in passing state of soft amalgam, it follows, that, in acquiring state, it absorbs at the same time 4·22 times its bulk and consequently the mercury, in pass- ing to t is increased in weight about 0·000 eriments of Mr. Davy it is incr twelve-thousandth. Our increase too is a minimum; for it is very possible, that a part of the ammonia is absorbed in the course of our experiment. Though this increase is very small, it would appear sufficient to explain the formation of the amalgam, if it be considered, that hidrogen and ammonia are very light substances; and that, being retained in this amalgam by a very weak affinity, they are scarcely more condensed than in the free state.

XI.

On the Decomposition of some vegetable or animal Substances subjected to the Action of Heat: by Mr. GAY-LUSSAC*.

Some substances partly decomposed by heat, partly volatilized.

WHEN certain substances belonging to the vegetable or animal kingdom, as oxalic acid, indigo, &c., are subjected to distillation, part is decomposed, and part is volatilized without alteration. To prove, that this is not owing to the impurity of these substances, we have only to distil anew what was volatized, and we shall find as much in proportion decomposed as the first time; so that, if the process be frequently repeated, we shall obtain a complete decomposition. These facts, though very remarkable, have

* Ann. de Chim. vol. lxxiv, p. 189. Communicated to the Society of Arcueil, November, 1809.

not

not sufficiently engaged the attention of chemists: I will therefore endeavour to explain them from the principles I have laid down in a paper on the volatilization of substances, printed in the first volume of the Society of Arcueil. The question to be solved is this: Why, when certain substances of the vegetable or animal kind are distilled, is part decomposed, and part volatilized? Why are they not entirely volatilized, or entirely decomposed? Why?

The substances, that present to us this kind of alteration are volatile, and at the same time capable of being decomposed by heat. Farther, a substance cannot be volatilized below the point at which its vapour has a degree of elasticity sufficient to overcome the weight of the atmosphere, unless this vapour can mix with the air, or some other elastic fluid. Volatilization effected by heat alone, or assisted by gas.

Now if a substance, that is both volatile and capable of being decomposed, be subjected to the action of heat, it may happen either that it will be completely volatilised, before it experiences a sufficient degree of heat to decompose it; or that it will be decomposed, before its vapour has acquired a sufficient elasticity, to overcome the pressure of the atmosphere. A substance may be volatilized without decomposition, or the contrary.

In the first case there is no difficulty: it is that of the distillation of acetic acid, alcohol, ether, volatile oils, &c. First case. As to the substances included in the second, as indigo, the oxalic, gallic, and succinic acids, wax, suet, fixed oils, &c. they begin to be decomposed, before they are volatilized: but, as their decomposition produces gasses, these gasses will cause the volatilization of the part not decomposed, in the same manner as the air causes that of water below its boiling point. Second. Cause of a partial volatilization:

Since the gasses that result from the decomposition of a substance are the cause of its volatilization, and withdraw it from complete destruction; and as all elastic fluids possess the same properties in this respect; it is easy completely to volatilize indigo, several vegetable acids, and many other substances, without their undergoing any alteration. It is sufficient, to keep their temperature a little below that at which they are decomposed, and to cause a current of some elastic fluid, that has no chemical action on them, to pass through them. which may be prevented.

These

These observations will be found, no doubt, capable of frequent application. It was from not being acquainted with them, that Mr. Chevreul has given an explanation of the action of caloric on indigo, which is by no means satisfactory.

XII.

Remark on Mr. Moore's *Paper on the Motion of Rockets. In a Letter from a* Correspondent.

To Mr. NICHOLSON.

SIR,

Remark on Mr. Moore's paper on rockets.

YOUR readers undoubtedly feel much indebted to Mr. Moore for some ingenious papers upon the motion of rockets. As the subject is an important and curious one, it is highly deserving of accurate investigation. On this account I am desirous of pointing out to Mr. Moore's notice, as early as possible, an errour into which he has inadvertently fallen. In his investigation of the resistance opposed to a cylinder moving in a fluid, in a direction inclined to the axis, he expresses the sine of the angle P T *n* (see fig. 2, Plate vii,) in terms of the sines and cosines of P T Q, and Q T *n*; forgetting that the three angles are in *different planes*, and consequently that the trigonometrical formula, to which he refers, will not apply.

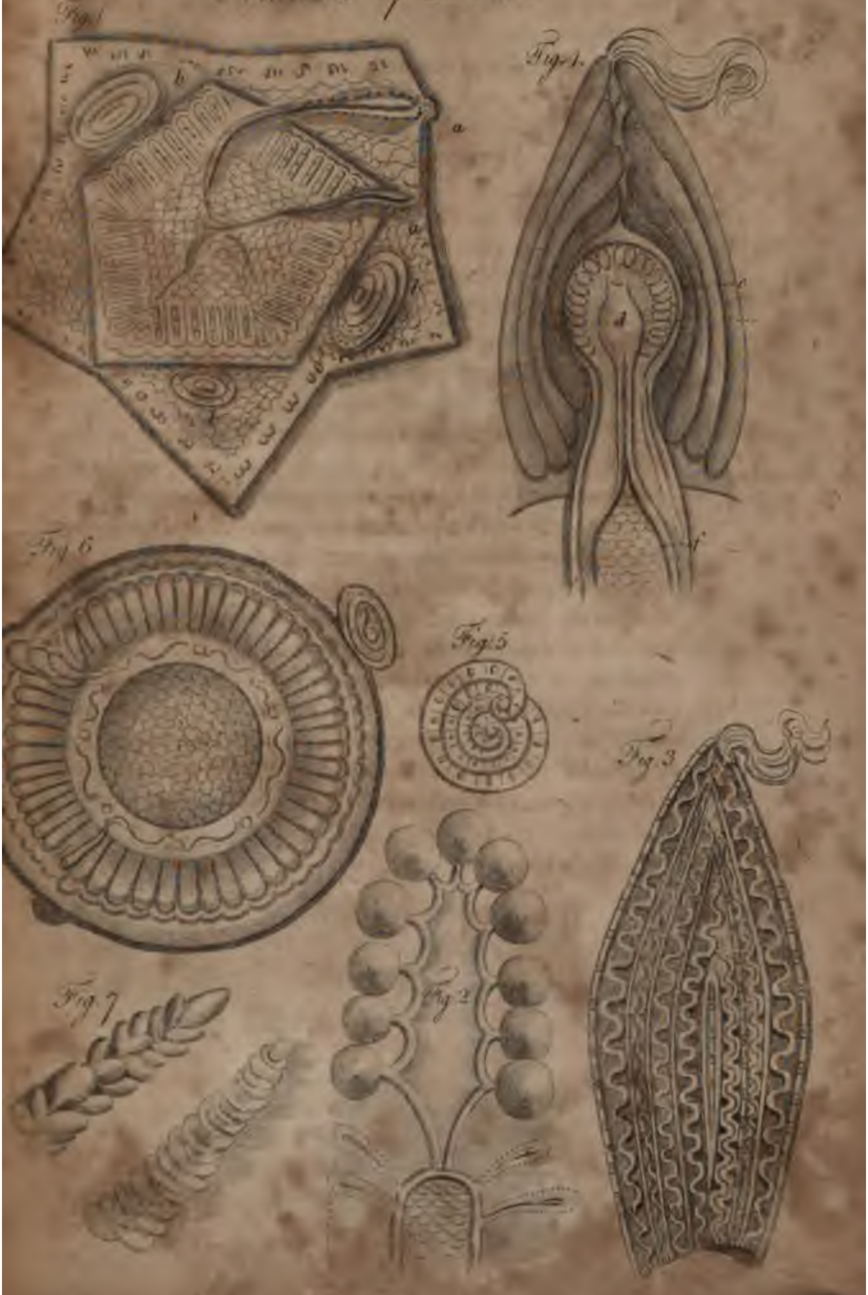
I am, Sir,

Yours, &c.

ZENO.

INDEX

Structure of Plants



EW YORK
C LIBRARY
ASTOR, LENOX AND
DEN FOUNDATIONS

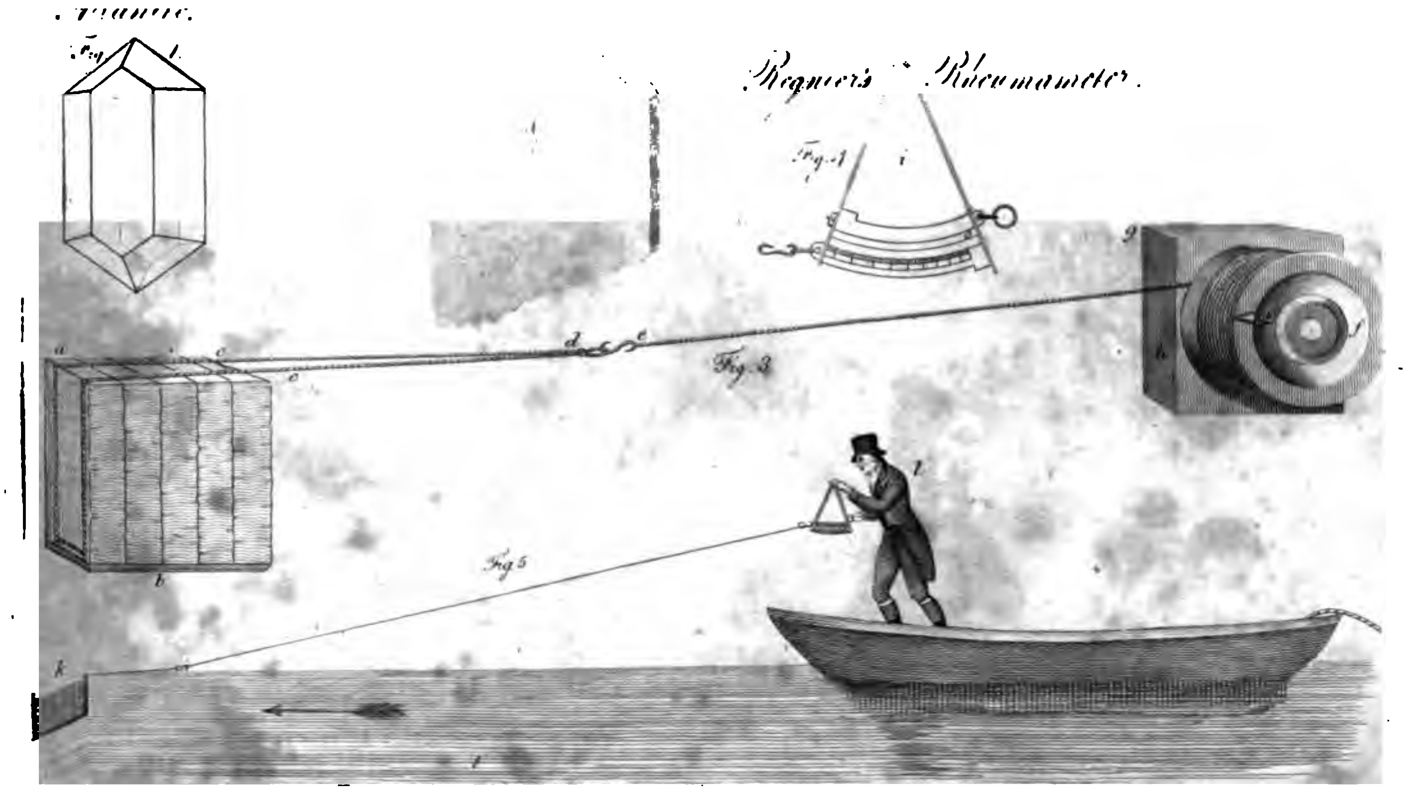


Fig. 1
Fig. 3
Fig. 5

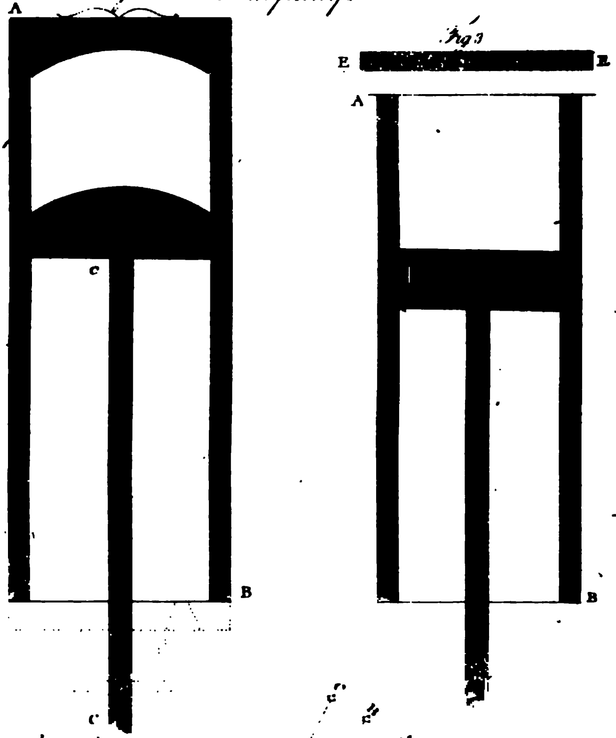
THE NEW YORK
PUBLIC LIBRARY
ASTOR, LENOX AND
TILDEN FOUNDATIONS

Barometer

Airpump

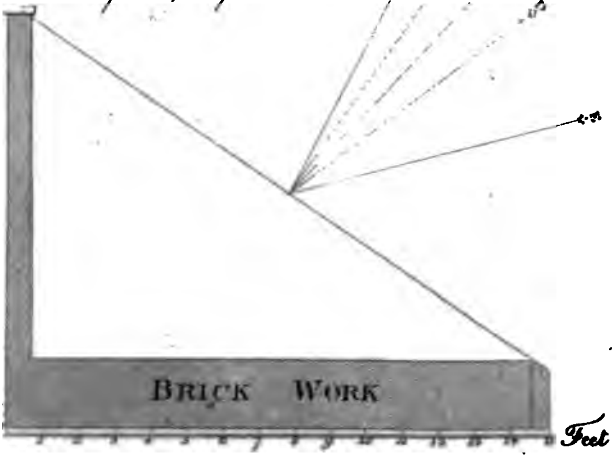
Fig. 2

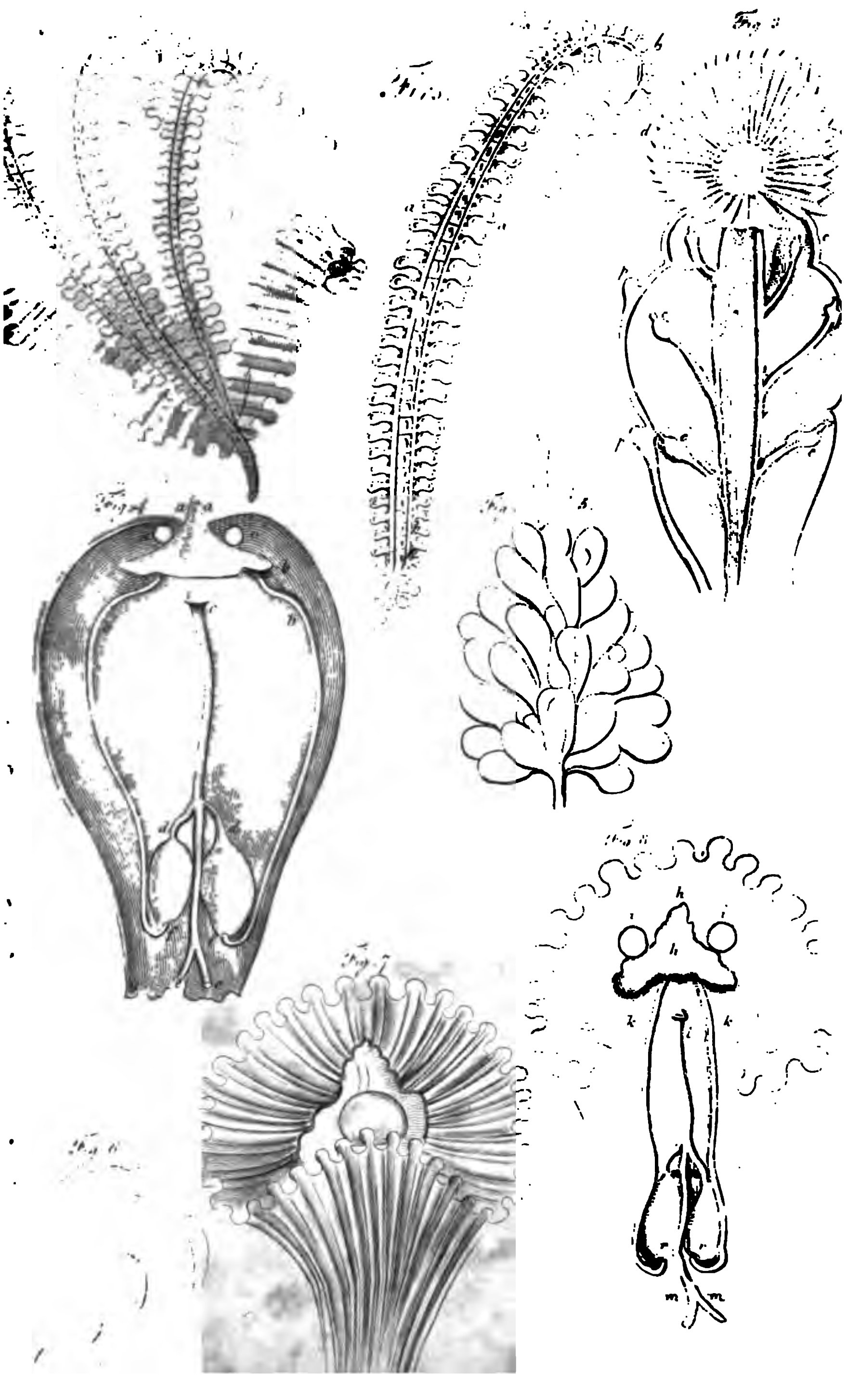
Fig. 3



...rcing House for Grapes

Fig. 4

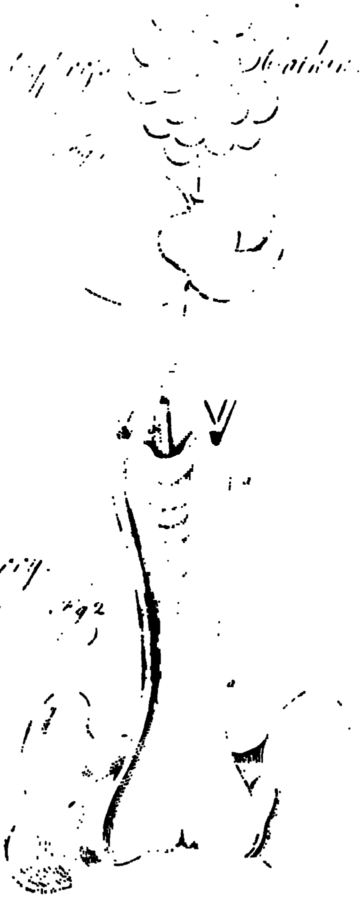




THE NEW YORK
PUBLIC LIBRARY
ASTOR, LENOX AND
TILDEN FOUNDATIONS

Barberry.

Fig. 2



[illegible] ARY
[illegible] K AND
[illegible] DATIONS

Mr Lee's Thrashing Machine


NEW YORK
LIBRARY
LENOX AND
FOUNDATIONS


Mr. I. Ball's Screw adjusting Plough.
Fig. 1.
Mr. Baker's Thistle Extirpator.
Fig. 2.
Mr. Jeffery's Expanding Harrows
Fig. 3.

THE NEW YORK
PUBLIC LIBRARY

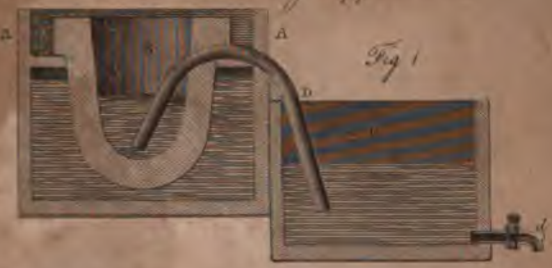
ASTOR, LENOX AND
TILDEN FOUNDATIONS

Mr. J. Davis's Fire Escape

ORK
RARY
LENOX AND
FOUNDATIONS

Mr. Moults Filtering Apparatus

Fig. 1



Mr. Smiths method of relieving a Horse which has fallen in the shaft of a loaded Cart

Fig. 2

Mr. Taylors Air Exhauster for Mines.

Fig. 3

THE NEW YORK
PUBLIC LIBRARY

ASTOR, LENOX AND
TILDEN FOUNDATIONS

INDEX.

INDEX.

INDEX.

Maycock,

N.

O.

P.

INDEX.

Smith,

END OF THE TWENTY-NINTH VOLUME.

Stratford, Printer, Crown Court, Temple Bar.

A

JOURNAL

OF

NATURAL PHILOSOPHY,

CHEMISTRY,

AND

THE ARTS.

VOL. XXX.

Illustrated with Engravings.

BY WILLIAM NICHOLSON.

LONDON:

PRINTED BY W. STRATFORD, CROWN COURT, TEMPLE BAR; FOR

W. NICHOLSON,

No. 15, BLOOMSBURY SQUARE;

AND SOLD BY

J. STRATFORD, No. 112, HOLBORN HILL.

1812.

PREFACE.

THE Authors of Original Papers and Communications in the present Volume are Mrs. Agnes Ibbetson; W. H. B.; Mr. John Davy; Mr. Grover Kemp; Thomas Forster, Esq.; Luke Howard, Esq.; Dr. Delaroche; W. Moore, Esq.; H. T. B.; Adam Anderson, Esq.; Marshall Hall, Esq.; Mathematicus; Mr. Charles Sylvester; Thomas Stewart Traill, M. D.; Mr. John Murray; Richard Lovell Edgeworth, Esq. F. R. S. M. R. I. A. &c.; John Farey, senr. Esq.; Nauticus; and Mr. John Gough.

Of Foreign Works, M. Berthollet; M. Gay-Lussac; M. Thenard; M. Vauquelin; M. Malus; M. Biot; Mr. J. Cloud; M. d'Arcet; M. Decandolle; M. Vitalis; M. Haüy; Prof. Pictet; Prof. P. Prevost; Count de Fourcroy; M. Berzelius; M. Guyton-Morveau; M. Klaproth; M. A. Laugier; M. Chevreul; M. Bucholz; and M. Martres.

And of British Memoirs abridged or extracted, George Pearson, M. D. F R. S.; Luke Howard, Esq.; the Rev. John Simpson; the Right Hon. Sir Joseph Banks, Bart. K. B. P. R. S. &c.; Mr. Bryan Donkin; Mr. J. D. Ross; Mr. George Marshall; James Smithson, Esq. F. R. S.; Mr. Richard Cathery; Everard Home, Esq. F. R. S.; Thomas Andrew Knight, Esq. F. R S. &c.; Mr. J. Hassel; Mr. William Corston; Dr. William Roxburgh; David Brewster, LL. D. F. R. S. Ed.; B. C. Brodie, Esq. F. R. S.; Mr. William Lester; Mr. William Salisbury; and Mr. Samuel Roberts.

The Engravings consist of 1 and 2. Various Figures representing the Hairs and minute Cryptogamiæ on Plants, greatly magnified and delineated from Nature, by Mrs. A. Ibbetson. 3. Mr. Donkin's Tachometer, for ascertaining the Velocity of Machinery. 4. A Hippograph, or Mode of conveying Intelligence by Cavalry. 5. Mr. Ross's Machine for separating Iron Filings from those of other Metals. 6. Mr. Marshall's Sash-frame for preventing Accidents. 7. Peduncles of Leaves delineated and dissected, to show their Mechanism, by Mrs. Agnes Ibbetson. 8. Apparatus to explain the Decomposition of Water in separate Vessels by Galvanism, by Adam Anderson, Esq. 9. Crystals of carbonated Lime, by Abbè Haüy. 10. Plans and Sections of a Spire of a new Construction, lately erected at Edgeworthstown, by R. L. Edgeworth, Esq. F. R. S. M. R. I. A., &c. 11. Diagrams for the Demonstration of the Fundamental Property of the Lever, by D. R. Brewster, LL. D. F. R. S. Ed. 12. Mr. Lester's Machine for washing Roots. 13. Mr. Salisbury's Method of packing and preserving Plants and Trees. 14. The Sheffield Apparatus for cleaning Chimneys without the Aid of Climbing Boys.

TABLE

TABLE OF CONTENTS

TO THIS THIRTIETH VOLUME.

SEPTEMBER, 1811.

OCTOBER,

[redacted]

CONTENTS.

OCTOBER, 1811.

NOVEMBER,

NOVEMBER, 1811.

DECEMBER,

DECEMBER, 1811.

SUPPLEMENT

SUPPLEMENT TO VOL. XXX.

INDEX

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

SEPTEMBER, 1811.

ARTICLE I.

On the Hairs of Plants. In a Letter from Mrs. AGNES IBBETSON.

To Mr. NICHOLSON.

SIR,

WE study the larger and conspicuous parts of botany, but we leave with a sort of contemptuous neglect all the more diminutive features, as unworthy our notice, little aware how much nature performs in this way, and how many great and powerful purposes are answered by apparently little means, extremely multiplied. If we minutely examine all the works of nature, this will appear a very important truth; nor does any art or science show this more conspicuously than the study of physiology, where all are multiplied little means, conducing to one great and important end. The subject of the present letter will peculiarly exemplify this. It is on the Hairs of Plants. Powerful purposes answered by little means extremely multiplied.

I have endeavoured to show, and I hope succeeded in proving, " that the idea of perspiration in plants is an absolute fable," originating from the poorness of our magnifiers: No perspiration in plants.

and that all that was taken for perspiration by botanists was one of two things: either 1st, a sort of hair, or instrument in that shape, for carrying water to the interior of plants, and performing many of those important services, which their diminutive appearance makes us overlook; or, 2dly, a sort of cryptogamian plant, wholly nourished by the dews of the atmosphere, and proving what they are by passing, like all other plants, from flower to fruit and seed, and showing in each various alteration the concomitant properties of each. That both these appearances have been taken for perspiration there can be no doubt, since I have repeatedly and regularly followed them in every plant peculiarly said to perspire much; and always found it either a fruit or an instrument: and instead of being bubbles of water issuing from the cuticle (as is supposed) their make alone would prove the contrary, as they could not transpire on stalks. Even the vineball is proved to have a stem; and is therefore an instrument, not a bubble. Before I give a more ample description of these, I shall adduce a farther objection to the idea of perspiration; and prove the impossibility of it by the disclosure of a discovery I long ago made, but would not give to the public, till perfectly convinced of its reality. I have already said, that there is found in the corolla of flowers, and in the stem of trees, a clear transparent skin, which, placed under the most excessive magnifier, shows no alteration of form, nor can any aperture be discovered in it. The same is found on the exterior of the cuticle, on each side of the leaf of all plants; so that it is not possible that a drop of water can pass to or from the interior in this way, though certainly air may. It is difficult to clear the skin from all the marks the pattern of the pabulum leaves on it, which were taken by all botanists for the pores in the cuticle. I was once of this opinion; but I have since with such excessive pains laboured to elucidate this subject, and to prepare for the microscope upwards of forty specimens, (cleared in the way described in a former letter;) which in this state were thoroughly examined by myself and others; that there can be no doubt of their being on both sides impervious to moisture. They are divided into small compartments by a narrow vessel; and so extremely fine is the skin

Source of the deception.

Another objection.

The exterior skin of leaves is without pores.

skin, that, when placed in the sliders of my solar microscope, it is only on turning the light in a particular direction while the eye is on it, that it can be discovered. But the double microscope makes it very visible, let it be ever so nicely cleaned and prepared. I am hardly acquainted with any part of the vegetable structure, that plays so many parts, and shows itself in so many ways, as this delicate skin. It was through this transparent skin I saw the dew drop enter the pabulum. It is probably the same skin of which the hairs are formed, which confine not only water but air.

This skin seen in every part of the vegetable structure.

How then can water enter the interior of the leaf, which is thus guarded on both sides by this transparent medium? that water which is often seen underneath the skin of vegetables, and wholly independent of the vessels? it is to the hairs alone they are indebted for it; which, however simple they may appear to the casual observer, are very far from being so in reality. To these indeed plants owe many of the most delicate and important offices, nor can a person see them once, and have a doubt remaining as to their being real instruments formed to effect some curious purpose. To give a faint idea of this astonishing subject is all I can attempt, for to collect a tenth part of the various instruments these hairs are intended to represent would be an endless labour; and to account for the use and manner of acting of a few is I fear more than I can perform well, or as I could wish.

Water enters the leaf through hair-like vessels.

The first idea that occurs on seeing these hairs greatly magnified is, that they resemble the instruments in an immense laboratory. But great indeed must be the laboratory that could show instruments of such contrivance, figures so various, and mechanism so astonishing, even putting their size out of the question. By the most careful attention to their forms, by filling them with coloured liquids, and with art and constant practice learning to manage the heat and light of my solar microscope (opaque as well as common), I have been able repeatedly to fill and empty a few of the instruments, and by these means understand something of their construction. But it is extremely difficult to get a liquid thin enough, as the most trifling degree of thickness chokes the valves. This was the case with extremely diluted ink: still it is to this I owe the conviction of the opening of the

They resemble the instruments of a laboratory.

valves being double. I have also by observation noted the hairs always allotted for certain purposes; but there are many uses it is not possible even to guess at. Innumerable indeed are the offices these hairs perform. To shade from light and heat, to convey moisture, to decompose water, to catch and secure the drops of rain as they fall, and select the dew from the atmosphere, I have often seen them do; but these, I conceive, are but a small part of the offices they daily execute: when an instrument is wanted for the several purposes of carrying moisture to the plants, catching the rain drops on their points, and defending the back of the leaf from the sun's rays, a simple kind of hair is generally used, particularly found on the leaves of trees, as represented Pl. I, fig. 1. This is merely a managed vacuum, which draws the water into the vessel, and thence lets it into the pabulum of the leaf. It is well known, that the backs of most leaves will not bear the scorching sun: and nature has peculiarly formed and adapted the spiral wire, to turn the leaf if so directed. It is not from any great difference in make, for both cuticles are most frequently alike on each side of the leaf; being both composed in part of this clear skin; but the one is pressed down on the pabulum, and is always therefore moist: while the other stands much above it, and, if heated, would soon dry up, peel off, and thus cause the decay of the leaf. When leaves are to be defended from heat alone, and no other purpose to be answered, then, (as in coltsfoot and many other very wet plants) the hairs are formed like a ribbon with a quantity of threads woven round them, and wholly without moisture. But in those which contain moisture, all the different pipes have at the bottom a contrivance for the entrance of the water into the pabulum. This perfect mechanical process I have several times witnessed and described as the dew drops entering the cuticle. See fig. 2, in which the thread *a* contracts or loosens to admit or retain the water. When from a long continuance of sunshine and dry weather in February or March, when the buds of trees are enlarging, and of course much humidity is required for their preservation, a quantity of hairs will be suddenly seen covering all the buds in various directions, the sun cracking the scales, and all the apertures filled

The various uses of the hair.

The reason leaves will not bear the sun on their under surfaces.

Occasional hairs bring

t.

filled by a quantity of vessels shaped as at fig. 3. For several years past I have attended with peculiar care to this phenomenon, and noticed the sort of instrument used on the occasion. It never varies, and regularly appears to select the dew from the atmosphere. By four or five in the morning they are almost empty; by eight, perfectly full; again emptied before noon, and late in the evening I have seen them replenished to bursting, or running over: but how they fill themselves, except by means of a vacuum, I have not yet been able to discover. This year all the trees (or rather the buds,) were covered with this vessel, owing to the long drought in March, which never fails to bring it on; it appeared as if all the buds were covered with diamonds.

moisture in droughts.

Buds of trees covered with occasional vessels.

In perfumed plants there is a species of instrument that baffles all conjecture as to the manner of its management, or the uses to which it is applied. This is represented at fig. 4. *e* forms a part of it, but is often found separate. The different bells bubble between each division (when part of it is turned to the sun) like a pulse glass when a warm hand is applied to one of the balls: on turning a very hot sun on these, I once blew up two of them; and it not unfrequently happens, that the quantity within the hair, if heat is suddenly applied, bursts the vessels: but it is fortunate when it does so, since they always break at the valve, and by this means discover much of their interior formation. These instruments are mostly found in the balm of gilead, the most perfumed geraniums, and plants that coincide in this respect. When I first saw this, and perceived the divisions to bubble, I was persuaded it was a decomposition of water; but was soon undeceived, for none of it disappeared. I have since repeatedly seen the effect, and been convinced, that it is similar to that which takes place in the pulse glass, and caused by the rarefaction of the air, and the increasing particles of liquid from the admission of caloric among them. Indeed every little power is visible here, nor can any instrument be so fit to try every little variation of temperature, moisture, or evaporation, as these most delicate diminutive ones, which are never idle, as long as the vegetable on which they are placed lives; sensible of every change, even Leslie's differential thermometer is quiet in comparison.

Hairs of odoriferous plants.

Hairs always break at the valves.

Hairs uncommonly susceptible of alteration.

Fig.

Hairs appropriated for the decomposition of water.

Fig. 5 is the one that appears constantly used for the decomposition of water, where it passes away in a few minutes, with its usual bubbling. I have often seen the same decomposition between the two glasses of my sliders, when exposed to a very hot sun; in short, it is a process so continually taking place, that you cannot make the proceedings of the vegetable world visible to the eye, without a perpetual recurrence of this chemical work: such a quantity of hidrogen is wanted, not only for the juices of the bark, but for the seed, inflated with it, that the process must of course be perpetually going on. In describing the various sorts of instruments I have observed, I have given two or three that strike as most singular; but they are in such numbers in plants, and so various, that I have found it difficult to select them. It is not uncommon to see several different sorts of instruments on the same plant, apparently appropriated to a variety of purposes, nor is it possible to mistake the fruit for the instrument: the latter so much resembles the purest crystal, and their forms are so extraordinary, their valves so truly mechanical, that no person can see them, and take them for any thing but what they are, "an instrument;" nor did I ever show them without exciting an exclamation of surprise. I have once or twice found them inflated with a green liquid; but this is very rare. This is the case in the love-apple. What in that plant was supposed to be perspiration is a small instrument of this kind (see fig. 6).

The cryptogamian fruit cannot be taken for the instruments.

The most extraordinary instruments found in small plants.

Extraordinarily figured hairs are rarely to be found, except in herbaceous annuals, or small plants. The wild plants exceed the cultivated in assistance of this kind. It would seem, that, when art lends her aid, nature is less attentive to the preservation of her nurslings: though I believe it requires many years cultivation to lose any of them, still I have found occasional hairs oftener on wild plants than on garden ones, and double flowers almost banish them. Trees and shrubs have seldom any but simple formed hairs, if those with double cases or valves deserve this epithet; but occasional assistance of this kind is perpetually found, particularly among exotic trees. Nor are hairs found often on evergreens; they would undoubtedly burst with the first frost of winter. The firs also are void of all assistance of this kind,

Extraordinary hairs seldom found in double flowers.

kind, except now and then on the calyx of the leaves: and then I have observed, that the water gets mixed with the resinous juice of the bark, which coagulating bursts the pipes. These hairs are also very different from those which are fixed to the flying seeds, &c., for they resemble the corallines, and the bones of fish; indeed the exact likeness of these three different objects is very striking and curious. The hairs which surround the buds of trees, and are generally wound round them, are never inflated till wanted, and till a certain time in the formation of the bud: when black (as in fraxinus excelsior, juglans regia, and many others), the valves are admirably seen to open and shut in a large magnifier, admitting and passing the water through the black lines.

That the hairs alter their forms, I have many proofs. During great drought I have seen those, which were before plain pipes, swell into divisions between the valves, changing their form from that at *e* fig. 7, to that at *f*; and plainly proving the shape of the valves to be as fig. at *g*. On placing fig. 8, Pl. II, in the solar microscope, after great bubbling and confusion, I took it out, and found the ribbon changed from the appearance it has at *h* to that at *i*. It appeared as if it had been before inclosed in another case, which case had melted away with the heat of the sun, and left the inclosed balls and string uncovered. I have so often seen the same result from repeatedly placing it, that I cannot doubt that this is the case. The divisions *k k* are often found attached to different shaped instruments, ending sometimes in bells, sometimes in plain pipes; contracted, or inflated, as the occasion requires. Nothing can be more common than fig. 9, which is always full of water; and fig. 10, which is found on the galium aparine. Extraordinary as is all I have related, it is not more wonderful than true. I am the first person that may be said really to have turned the solar microscope on the botanical world; is it then incredible, that I should have wonders to relate? did any person ever take a miscroscope in hand without it?

Hairs alter their forms.

I shall now turn to the cryptogamian plants, equally taken for perspiration, and described by all botanists as such. Many of them resemble the powdered lichens, when they begin

Description of the cryptogamian plants.

to go to seed, though at first appearing like a drop of water, which, even while your eye is on it, turns white, and soon becomes hard and firm; changing to seed. These are found on the mint, the pea, and innumerable other plants, said to perspire much. That which Hales took for perspiration on the leaves of the sunflower is a sort of mushroom, extremely moist, shown at fig. 11, *w*; and that on the vine, fig. 12: but I must stop, or my sketches would never end. I observe that the cryptogamian plants on the rose, and many other plants, because red, are allowed not to be perspiration: but surely the proof is not in colour, but on the matter passing from flower to fruit and seed, which all this sort does in a day or two; yielding generally a sort of sirup, and equally nourished by the dews of the atmosphere: and certainly equally unfit with the hairs to be reconed perspiration. I flatter myself therefore, that this will serve to convince those who still doubt.

Fruit known by its various changes ending in seeding.

Innumerable offices the hairs perform.

If I were to mention all the different offices to which the hairs are applied, it would be endless. To catch, convey, and mix, the powder of the stamen with the sirup of the pistil, they are peculiarly adapted, having in each hair a duct for conveying the mixed juices, when melted, to the canal in the pistil. All this is plainly seen, since in the solar microscope each hair is as large as a walking stick. How many various offices do the hairs perform in the corolla, calyx, and stipula! There is one peculiarly appropriated to this latter part, in all diadelphian plants, most curiously formed. How wonderful is the hair in wet plants! placed to guard the air vessels from being filled with insects, they exactly resemble swords, shoot in a circle and meet in the middle of the vessel as at fig. 7. How many an insect and water-fly have I seen run through by them! But this is not all, they have a sort of spring, which makes the hair strike down, and thus get rid of the creature it has threaded. When I give my letter on water-plants, I shall show the mechanism of this hair, which is as wonderful as any of the preceding account.

Different from the armature of plants.

This subject should not be made to interfere with the armature of plants, which is wholly of a different nature, and consists but of two sorts of thorns; the 1st like those of the rose, the acacia, the gooseberry, &c., is formed entirely of the

he rind, and an excrescence of it; probably arising from an extreme tendency in that part to grow in the same manner as the quercus suber, the ulmus campestris, and many others, the rind of which is a sort of cork, always increasing. The 2d sort of thorn is that which in the cratægus is a disorder in the tree, to which some plants are peculiarly subject: a sort of missed bud, from the stoppage of the line of life, caused probably from the momentary check of the juices, on some sudden alteration of the weather; as I have observed, that, when the barometer and thermometer are without much variation, except the natural one of day and night in the latter, no thorns come out. I have measured at such a time a shoot three quarters of a yard long, without a thorn. But when in the spring alterations are frequent, the branches will be scarce two inches, and always ending in a long one: and on dissecting this, the line of life will be found to have stopped, before any other part of the plant.

I intended to give merely a sketch of this subject, till I better understand how to inflate the hairs with a coloured liquid, and till I can more thoroughly comprehend their uses and management; for this indeed I should have waited, but that it was absolutely necessary to prove, that I would not have written against the perspiration of plant, without a complete conviction of the truth of my assertion: "that the whole system of perspiration could not be supported against the absolute proof the solar microscope adduces of its falsehood." If I were rich, I would certainly have the instruments imitated in glass, properly magnified (if it could be done) as I think much might be learnt from it. It is the mechanism of nature: we talk much of its simplicity, but it surely consists only in not making use of more contrivance than is necessary; and when the mechanic powers are wanted, can we do better than study them from models so perfect, forms so wonderful? and though we could not succeed in forming a sort of air pump in a hair; yet it might serve to teach us to simplify our machines, and to rectify many of our mistakes.

Hope to introduce the subject more in detail.

Your obliged servant,
AGNES IBBETSON.

Cowley Cottage,
July 29th, 1811.

II.

II.

Inquiry concerning the Natural Economy of Ants. In a Letter from a Correspondent.

To W. NICHOLSON, Esq.

SIR,

Inquiry concerning the natural economy of ants.

HAVING always observed, since I first commenced taking your excellent publication, that you have constantly paid the kindest attention to the inquiries of such as wish to be informed on the interesting subjects embraced by your plan, I feel almost confident of your permission to request, through the medium of your journal, the communication of such original facts and observations relative to the natural economy of different species of ants, as may have occurred to the notice of any of your numerous readers. It appears to me, that, striking as the habits of this genus of insects certainly are, the subject is, as yet, by no means generally well understood.

Your compliance with my request will be considered a particular favour.

I am, Sir,
Your obedient humble servant,
W. H. B.

III.

Report of a Committee, consisting of Messrs. Berthollet, Chaptal, Vauquelin, Le Breton, Vincent, and Guyton-Morveau, appointed by the Institute to inquire concerning the Process of the late Mr. BACHELIER, *for the Composition of a preservative Stucco**.

Speedy alteration of the stone used for building at Paris

IT was in 1755 that Mr. Bachelier, struck with the speedy alteration of the stones employed in the principal buildings at Paris, and the inconveniencies of the process employed from time to time to renew their surfaces, proposed to the super-

* Abridged from the Mag Enc. Dec. 1809, p. 241.

superintendant of the royal buildings to try a preservative stucco. Accordingly three pillars in the court of the Louvre were coated with this stucco for half their length, two facing the south, the other the west. These were still remarkable in july last for the uniformity of their tint, strongly distinguished from the dull gray and earthy aspect of the contiguous parts: but as the alterations made in completing the Louvre would necessarily destroy every trace of this experiment, the Institute appointed a committee to inquire concerning it, before it should be too late.

prevented by a composition,

In company with Mr. Fontaine, architect of the Louvre, the gentlemen abovementioned examined the pillars, and found, that the stucco applied formed a coat too thin to injure the finishing of the most delicate sculpture; that it retained a uniform colour even in the parts exposed to the action of the wind, rain, and sun; that rubbing it with the hand made no impression on it; and that, if one of the three pillars exhibited a reddish yellow tint, there could be no doubt, from its appearance in other respects, that this was owing to some colouring matter added intentionally.

too thin to injure the finest sculpture,

and unaffected by the weather.

It could not be found on inquiry, that Mr. Bachelier had consigned his process to writing, and the following was the best account his son could give of it from memory. "Its basis consists of the sifted powder of oystershells, previously washed and calcined to whiteness, mixed with the butyraceous and caseous part of milk. My father used the common cheese known by the name of *fromage à la pie* [skimmed milk cheese?]. He first separated all the wheyey part by pressure, and then left it some time exposed to the air to dissolve or soften. In this state he mixed with it a quantity of calcined oystershells in fine powder. When this mixture was brayed on a stone, the cheese softened, and formed a very smooth and whitish liquid paste. To make the stucco he diluted this with a solution of alum in water; the quantity of water being proportioned according to the thickness of the coat intended to be applied."

Account of it from memory by the inventor's son,

Mr. Bachelier could say nothing of the proportions of the ingredients, he only added, that, his father having thought of employing this composition undiluted to cover leaves of paper, from which writing was easily effaced by a wet

Paper coated with it from which writing could be effaced.

wet

wet sponge, he observed, that the oystershell powder was taken at random, and added to the cheese till it had acquired the consistency of a paste capable of being spread on paper.

Oxide of lead in it.

The committee having obtained from Mr. Bachelier a few leaves of paper covered with thin paste, found from the very deep black immediately given it by the hidrosulphuret of potash, that it contained a considerable quantity of oxide of lead, the presence of which there was no reason to suspect in the preservative stucco, so that they could not be considered as the same.

Analysis of the stucco.

It remained therefore to analyse the stucco, which was done by Mr. Vauquelin; though, as a very small quantity only could be obtained by scraping the pillars, it did not admit of repeated trials. The results of his analysis gave

Carbonate of lime	63
Sulphate of lime	7·73
Carbonate of lead	6
Oxide of iron, about	4
Silex	2
Water	20
Organic matter, an indeterminate quantity	
	102·73

The surplus of 2·73 Mr. Vauquelin ascribes either to the matter not having been dried to the same degree, or to the escape of a little carbonic acid during the calcination.

No animal matter found, but the pungent smell of burning vegetables emitted.

The presence of animal matter was sought for, but not a particle could be separated. The smell it emitted during calcination no way resembled that of aminal matters; on the contrary it had the pungent sharpness of vegetable substances. On being exposed to the action of heat in a retort however, a clear and almost colourless liquid came over, from which potash expelled a very evident ammonical vapour. This indicates, that some animal substance entered into the composition, but that in time it was decomposed, and left only an ammonical salt. The brownish colour it acquired in the fire also proves, that some animal matter still remained in it; though altered in its nature, since it neither emitted the smell proper to such substances, nor yielded any preceptable quantity of oil. Lastly,

Indications of animal matter.

Lastly, this matter yielded no appreciable quantity of alumine, so that it may be presumed no alum was employed in the composition. No alumine.

Mr. Bachelier having some of the paper that had been prepared by his father, the coating of this was analysed, and the result indicated, that Analysis of the paper coating.

Quicklime	56·66
Calcined gypsum	23·34
Ceruse or carbonate of lead	20

had entered into its composition.

On these proportions more dependance can be placed than on the former, since it was impossible to detach the plaster from the pillars without some of the substance of the stone itself. This more to be depended on.

That the caseous part of the milk is the proper vehicle for the powders we learn from the positive testimony of Mr. Bachelier, the son; and its utility is confirmed by the experiments of Mr. d'Arcet published some years ago*. Cheesy matter the vehicle.

Of the efficacy of Mr. Bachelier's composition there can be no doubt, as we have irrefragable and still existing testimony of it; nor would it be difficult to estimate this beforehand, when we consider the causes, that produce the gradual decay of the finest buildings in this capital, and the means of guarding against them. Efficacy of the compound unquestionable.

Hard and fine grained calcareous stone, susceptible of a greater or less degree of polish, is not liable to this alteration. It is therefore owing to the nature of the stone commonly employed, which is of a loose and unequal texture, filled with cavities, and found by analysis to contain 10 or 12 per cent of silex, and frequently 3 or 4 of oxide of iron. The difference of the stones from the quarries near Paris is evident from the tables of Mr. Rondelet, in his Treatise on the Art of Building; where we see, for example, that what is called the *grignard* of Passy is of the specific gravity of 2·462, and supports a weight of 6750 kil.; while the *lambourde* of St Germain has only 1·560 sp. grav., and is crushed

Stone not liable to this alteration, and liable to it.

* Déc. phil. an X, No. 5. The pamphlet entitled *l'Art de peindre au Fromage, ou en Ramekin*, which Mr. d'Arcet regrets his being unable to procure, was foreign to the subject, as it related to painting with soap of wax. [See Journal, Vol. I, p. 212.]

crushed by a weight of 921 kil. The prices of these two kinds of stone differ too in the proportion of 26 to 10.

Spiders form their webs on these stones.

It is not at all strange, that the little spider called by Linnæus senoculata, the cellar spider of Geoffroy*, should find on the surface of this stone a convenient situation to shelter itself, deposit its eggs, and spread the nets in which it awaits its prey. Its web extends circularly round the cavity, that serves as its den, forming round spots of 3 or 4 cent. [1 in. or 1½] radius. It is not thirty years since the *hotel des monnoies* was built, and I have counted no less than sixty-eight of these dark gray spots on one of the pillars of the vestibule. Similar ones are found not only on the stone, but on the coatings of plaster, and on the walls covered with common stucco. It is particularly in the joints and angles, that the insect begins to fix itself. I have seen several on walls, the stucco of which had been coated afresh within less than seven years. These spots at length form a continued coat, retaining the sloughs of these insects, the remains of those on which they feed, and the dust raised by the wind, so that lichens soon take root in them.

Mode of preventing this.

If it be asked, how is this to be prevented? the answer is easy. By a composition that resists water, will adhere to the stone so as not to scale off, has a sufficient degree of consistency to stop the pores accurately, is liquid enough to be spread as a wash, and uniformly to ice over, as it were, all the saliant and indented parts, without thickening the angles or blunting the edges, and lastly which gives to the assemblage of coarse grains the smooth surface of polishable stones, in which it appears these insects cannot nestle. And this we think may be expected from Mr. Bachelier's stucco.

Other means.

Meantime I must observe, that, in the present state of our chemical knowledge, other means of fulfilling these conditions may be pointed out. We know for instance, that phosphate of lime is one of the most fixed combinations: it would be sufficient therefore, to wash over the stone with phosphoric acid more or less diluted, or with phosphate of lime, lead, magnesia, &c., held in solution by an excess of their

* Mr. Latreille informs me, that he has found the same habits in Lister's *aranea atrox*.

their acid, to give it a sort of covering, that would render it as unalterable as the stone of Logozan in Estramadura. It is equally known, that sulphate of barytes resists all agents in the humid way; and we might certainly coat the stone with this earthy salt, by first impregnating it with a solution of sulphate of iron, zinc, magnesia, alumine, &c., and immediately washing it over with barytes water *. The insolubility of oxalates and tartrates of lime, and the adhesion they contract by deposition even on polished substances, suggest processes for washes not less solid; as the acids added to these salts to render them temporarily soluble, saturating themselves with their base from the substance of the stone itself, would not fail to connect together all the grains, fill up their intervals, and completely close the pores. Trials made with a view to ascertain the justice of this reasoning have confirmed the expectation of a successful result; since on the most porous stones they have produced a surface, on which the eye could see no appearance of coating, but which, being rubbed with wet black cloth till the cloth showed signs of wear, was not in the least soiled by it.

Preparations of this kind however would be much more expensive than Bachelier's stucco, so that their use must be restricted to the preservation of sculpture of extreme delicacy. The latter too expensive.

For farther satisfaction trials have been made with different kinds of stone, and stucco made in imitation of Bachelier's. These have given rise to the following observations. General results of experiments.

1. All the compositions in which alum water was employed soiled the fingers, and were washed off by water.

2. The cheese that acquires the greatest consistency with dry substances is that which is almost entirely deprived of the butyraceous and wheyey parts. Mr. d'Arcet, in the paper already quoted, had remarked, that these were more detrimental than useful, that painting with milk would not resist water, and that the cheese called *fromage à la pie* might

O

* Accident furnished Mr. d'Arcet with a striking proof of the readiness with which this change of bases by superior affinity will fill the pores of the most porous stones. A capsule full of strontian water happening to be overturned into a filtering stone, it never after let through a single drop of water. Filtering stone spoiled by accident.

be

be used after it had grown dry, though less advantageously than when fresh made and well drained.

3. A mixture of this cheese with lime simply forms a paste, that adheres but slightly even to coarse grained stone, and not at all to paper.

4. Calcined gypsum, which in a small dose facilitates the union of the lime and cheese, renders the paste hard and clotty, if it be used in too large proportion.

5. It had appeared, that whiting, which is used in paper hangings, might be admitted into the preparation: but it was found, that, if this earthy substance, which in a process described by Mr. d'Arcet is carried to twenty times the weight of the lime, may be used with success for inside work, it would make too thick a coat, and would not adhere so strongly to the stone.

6. The addition of a very little ochre, or red oxide of iron, to this preparation, will give it such a tint as may be wished, without altering its properties.

Proportion of cheese.

The proportion of cheese must depend in some measure on the state in which it is, and cannot be determined precisely but by the condition of making a soft paste. A fourth of the weight of the solid matters appears to be a sufficient quantity of cheese fresh from the press.

Preparation of the compound.

The quantity of lime to be used at once being determined on, it is to be slaked in as little water as possible, but enough to make it pass through a sieve not very fine, in order to separate the parts that will not slake. This is to be triturated with the cheese to the consistence of a soft, smooth, and coherent paste. To this are to be added the calcined gypsum and the white lead, which must not be adulterated with chalk, and by farther grinding on the stone with a little water the whole is to be reduced to a pap, rather thick than fluid. Lastly it is to be diluted with common water the moment of using it, which is to be done with a painter's or varnisher's brush.

IV.

IV.

Observations and Experiments on Pus. By George Pearson, *M.D. F.R.S**.

CHEMICAL writers vary in their statements of the properties of pus; and they consider, that a farther investigation is requisite for the purposes of science. Physicians confess, that, in numerous cases, they cannot form a satisfactory judgment of the nature of diseases, on account of not being able to determine what is, and what is not purulent matter; likewise probably, on account of the existence of different kinds, or varieties, at least, of this substance, afforded by different disorders. Properties of pus not determined.

I beg leave, therefore, to submit to this learned Society, my own observations, experiments, and reasoning on this animal matter.

Section I. *Simple, and obvious Properties.*

The different kinds of fluid, commonly considered to be pus, may be distinguished by the following titles: Different kinds of pus.

I. The creamlike and equally consistent.

II. The curdy and unequal in consistence.

III. The serous and thin kind.

IV. The thick, viscid or slimy.

I. A pint of the first sort was taken out of the pericardium, after a fatal inflammation of the heart, in St. George's Hospital, and obligingly sent to me by my colleague, Dr. E. N. Bancroft. Properties of the 1st kind,

The *colour* was yellowish—the *smell* was fleshy when warmed—it was smooth and unctuous to the touch.

2. The specific gravity of two different portions was as 1630 and 1633, that of distilled water being 1580; each substance being of the same temperature. Serum of the blood of different patients, was found at the same time to be 1626, 1627, and 1630. Accordingly, the distilled water being 1000, the pus is 1031, and 1033; and the serum is 1029, and 1031.

* Philos Trans. for 1810, p. 294.

after 12 hours repose, about two ounces by measure ... mpid fluid having appeared on the top, it was decanted from off the opaque purulent fluid; which was become thinner in the upper part of the vessel containing it, and thicker in the lower than before.

4. On farther repose, it did not become offensive so soon as a portion of the same pus mixed with a little blood, or as serum alone.

5. This pus neither indicated acidity nor alkalescency to the usual tests, viz. turnsole paper, tincture of red cabbage, Brazil-wood paper, and turmeric paper. I have, in other instances, sometimes observed acidity to be indicated by turnsole paper; but in none alkalescency, so long as the matter remained without fœtor.

6. Being examined under the microscope, when duly diluted with distilled water, innumerable spherical particles were seen, which did not appear altered in figure, or diminished in number, by extreme dilution; that is, they did not appear to have been dissolved.

of the 2d kind, II. A pint of pus of the second kind, viz. *curdy*, was afforded by a psoas abscess.

The colour was brown. It felt knotty. On pouring from one vessel to another, the curdy masses were manifest, and of various sizes, from that of a pin's head to a hazel nut. It was more viscid than the former, and of a little greater specific gravity. On standing, a limpid fluid appeared upon the top, as in the first kind, but in smaller quantity. Globules were seen with the microscope, but also a number of irregularly figured larger masses. Putrefaction took place sooner than in the former kind. In other properties, this pus was similar to the first kind.

of the 3d kind, III. *Serous thin pus.* It was produced by a fatal inflammation of the peritoneal coat, without ulcer, and taken out of the cavity of the abdomen. A good deal of serum was also effused, of which the pus was a deposit. It was not much thicker than milk. To the feeling it was not at all unctuous. The smell was slightly offensive. On standing 24 hours a sediment appeared, occupying only one half the full vessel, under a wheylike liquid. Putrefaction took place sooner than in either of the two former kinds. The specific

specific gravity was the same as that of the first sort. In other properties it was similar to the creamlike pus above distinguished.

and of the 4th.

IV. A pint of the *viscid pus* was obtained from an abscess among the muscles of the thigh. If I had not had entire confidence in Mr. Brodie's accuracy, who was so obliging as to attend to my request, on this and many other like occasions, I should have supposed, that this was expectorated matter, it so exactly resembled in its simple properties the *ropy kind*, described in a paper on expectorated matter. Phil. Trans. 1809, P. II, p. 317*.

The appearance was not quite uniform, there being semitransparent masses in small proportion, mixed with the perfectly opaque white matter. It was almost inodorous. To the touch it was quite smooth. The specific gravity was nearly that of the second kind of pus.

On standing 24 hours, about one ounce measure of limpid fluid rose to the top of the whole mass. Putrefaction did not take place so soon as in expectorated matter of the same consistence.

The examination by the microscope manifested innumerable spherical particles among leafy masses, and numerous particles of irregular forms.

The simple properties were otherwise similar to those of the other sorts of pus, above distinguished.

Other differences.

Many other differences of purulent matter are universally recognized; but they are either varieties of the four kinds already named, or the differences depend upon the obvious mixture with adventitious substances; such as the red part of the blood, coagulated lymph, serum, putrefied matter, fibrous and membranous masses, calculi, &c.: therefore, I deem it useless to describe them.

Sect. II. *Agency of Caloric.*

Action of heat on pus.

1. The above kinds of pus coagulated like serum of blood, into a firm, uniform, soft solid, at the temperature of 165° completely; but partially at 160° of Fahrenheit's thermometer.

* See Journal, Vol. XXV, p. 290.

The decanted limpid fluid from pus, Sect. I,—I, II, IV, coagulated completely into a firm uniform mass, like serum of blood, at 165°, but it became opaque and thickened at 160°. By pressure of the firm curd thus produced, a watery liquid was separated, which on due evaporation did not give a jelly, but was coagulable like the decanted liquid just mentioned.

The thick opaque matter, after decanting the limpid fluid, coagulated as before said, into a firm mass at 165°.

Evaporated to dryness.

3. Each of the above four kinds of pus, being evaporated to dryness, left in no case less than one tenth of its original weight, or more than one sixth; but most frequently one seventh or one eighth of brittle matter. The smallest proportion of residue was left by the 3d, or serous kind; the largest, by the 2d or curdy. These residues generally became rather soft, especially those of the 3d, or the serous kind, after exposure to the air.

4. The opaque part of pus after separating the limpid fluid afforded on evaporation from $\frac{1}{15}$ to $\frac{1}{10}$ more of brittle residue, than an equal weight of the pus itself; and it remained hard on exposure to the air. The limpid fluid, evaporated to dryness, yielded about one tenth of brittle residue; which grew moist, and sometimes deliquesced, on exposure to the air.

Residua.

5. The brittle residues above mentioned (3), being exposed to fire in platina crucibles, flamed for some time, emitting a very offensive, pungent, empyreumatic smell; the uninflammable residue being kept in a state of ignition for a longer period, what remained at length was fused readily from the *serous*, *viz.* the third kind of pus; but in the cases of the other exsiccated residues of the 1st, 2d, and 4th kinds of pus, they barely were melted, or only became soft and claggy. The fused residues from the *serous pus* amounted to $\frac{1}{10}$ or $\frac{1}{11}$ of the exsiccated pus; and to $\frac{1}{130}$ or $\frac{1}{100}$ of the original purulent matter. Those from the second kind, the *curdy*, amounted to $\frac{1}{50}$ or $\frac{1}{60}$ of the dried matter, and to $\frac{1}{350}$ or $\frac{1}{400}$ of the pus itself. The fused masses from the 1st and 4th kinds of purulent matter afforded intermediate quantities of melted matter between those just mentioned.

6. The

6. The fused residues (5), being treated in the manner described in a former paper, Phil. Trans. 1809, P. II, p. 326—329 *, I found they consisted chiefly of muriate of soda, phosphate of lime and potash; with strong indications of carbonate of lime, and a sulphate; beside traces of phosphate of magnesia, oxide of iron, and vitrifiable matter, probably silica. On a reasonable calculation, it appeared, that in the *serous kind* of pus, the muriate of soda amounts to from one and a half, to two per 1000; the phosphate of lime from one, to one and a half per 1000; the potash from one half, to three fourths of a part in this quantity; and the other matters together, to half a part in 1000. In the *curdy matter*, the second kind, the muriate of soda amounts to from three fourths of a part, to one in 1000; the phosphate of lime to one; the potash to less than one half; and the other matters united, to half a part in 1000. The first kind of pus, the *creamlike*, and the fourth, the *viscid*, afforded from the melted residue the same substances as the *serous* kind, excepting a somewhat smaller proportion of muriate of soda, and potash.

7. The brittle residues of evaporated pus, after decanting the limpid fluid (4), being treated with fire as above related, the remaining matters were melted with more difficulty, and less completely, and contained a smaller proportion of muriate of soda and potash than the original pus.

8. The decanted limpid fluids (4), being evaporated to dryness, these residues were exposed to fire. They were melted, and then afforded a larger proportion of muriate of soda and of potash, than the pus itself; but with the same proportion of the other saline and earthy substances.

Sect. III. *Agency of Water.*

1. After decanting the limpid fluid from off half a pint of the four kinds of pus as above related, (Sect. I.) three ounces by measure of distilled water were mixed with each of them. After 48 hours repose, a limpid fluid of nearly the quantity of two ounces by measure was seen Action of water on pus.

* Journal, Vol. XXV, p. 227—229.

forming

forming an upper stratum to the pus. It was decanted for examination.

(*a*) On exposure to fire it became turbid like milk, as soon as the temperature was elevated to 165°, but did not become thicker at a greater elevation.

(*b*) On evaporation to dryness, the residue amounted to about one fifteenth of the weight of the liquid from the serous pus, and to one twentieth from the three other kinds; in place of about one tenth, as from the first decanted liquid, (Sect. I, 4); and as from serum of blood. The residuary matters were of the same kind as those above described, Sect. II, 2—6.

2d solution. (*c*) Three ounces by measure of distilled water having been again mixed with each of the four kinds of pus, and, in 48 hours, two ounces measure of decanted limpid fluid from each having been evaporated to dryness, residues of the same kind, in the same proportions, and in nearly the same quantities as before, were obtained (*b*). These decanted fluids became nearly as turbid as the former, on raising their temperature to 165°.

3d solution. (*d*) Distilled water was added a third time, in the quantity of eight ounces by measure, to each of the four parcels of pus under examination; and, after 48 hours repose, six ounces of limpid fluid were poured off from each of them. At the temperature of 165°, the decanted fluids became turbid; that of the serous pus more so than the others. On evaporation to dryness, a much smaller quantity of residue was obtained than before, viz. one sixtieth from the serous pus, and one seventieth from the others; and it consisted of the same kind of substances as above described; but the muriate of soda and potash were in smaller proportion than before.

4th solution. (*e*) A fourth time distilled water, in the quantity of a pint, was mixed with the present four parcels of pus; and, after standing 48 hours, three fourths of a pint of clear colourless liquid was poured off from each of them. It became slightly turbid and whitish on boiling. On evaporation, each parcel afforded about $\frac{1}{50}$ of the fluid employed. The residues now consisted of animal matter, with a much smaller proportion than before of muriate of soda, phosphate

phosphate of lime, and potash—nothing else could now be traced.

(*f*) Distilled water, in the quantity of a pint, was once more mixed with the four sorts of purulent matter undergoing inquiry. After 48 hours, a pint of liquid was decanted from off each of them; but being slightly turbid, they were left to stand 24 hours. By this time a sediment was deposited from each of the liquors; but being still, though very slightly, turbid, they were filtrated through suitable paper. They were then transparent. The transparent filtrated liquors had their transparency disturbed by a boiling temperature. They became also slightly milky with nitrate of silver, but scarcely so with infusion of gall nut. On evaporation to the quantity of an ounce from each pint, the residuary liquids appeared slightly globular. These, on evaporation to dryness, yielded not more than one part of animal matter, from each 500 of the transparent filtrated liquids. 5th solution.

(*g*) On standing three or four days in a cold room, the parcels of pus, after the ablutions just related (*a*—*f*), exhibited a whey coloured liquor at the top, of which about ¾ of a pint was poured off from them. More turbid liquor was also separated from the washed pus, by pouring it upon a porous cotton cloth strainer, which left purulent matter of the consistence of starch mucilage, amounting to about one half the original weight. Residuum.

(*h*) The pus freed from coagulable limpid liquid by repeated ablutions (*a*—*h*) was white as snow—equal in consistence—perfectly smooth—the 4th kind was less viscid than before, but the others were more so—no smell—not at all disposed to putrefy—on elevating its temperature to 165° and higher, it did not coagulate into one mass, nor into clots, or large masses of curd, but a watery fluid separated from a fine soft somewhat curdlike opaque fluid; which did not become more curdy, even on boiling—it did not appear that above a grain of this part, or state of pus, dissolved in 1000 waters—was highly globular under the microscope, and remained so, although coagulated by nitrate of silver; by infusion of gall nut; by alcohol; and supersulphate of alumina—with muriate of ammonia, nitrate of potash, and other neutral Its properties.

...al salts, and with carbonate of potash, it produced a ...scid semitransparent mass like expectorated half transparent matter—exposed to fire in a platina crucible, it was inflamed, but did not emit an offensive smell, and after continuing the ignition, the residue was a particle of half fused matter, not amounting to $\frac{1}{1000}$ of the pus after ablution, nor above $\frac{1}{500}$ of the same matter exsiccated; it consisted of phosphate of lime and vitrified matter—no ammonia was perceivable, on mixing lime with this washed pus; nor muriatic acid on adding sulphuric acid.

Different kinds of pus agitated in water.

2. (*a*) A tea spoonful of the *creamlike pus*, being agitated in half a pint of distilled water, produced a milky fluid, with a number of small curdy particles suspended, but very few leafy or fibrous pieces or clots.

(*b*) The *serous pus* being treated as just mentioned (*a*), the same appearances ensued.

(*c*) The *curdy pus* being agitated in the same manner in water, a number of clots, leafy, and fibrous masses, were seen suspended among fine small curdy particles in a pearly liquid.

(*d*) The *viscid pus* being treated as just said, it required long continued and violent agitation, to diffuse it through the water, and then the appearances were as last described.

Boiled.

3. Pus of any kind, after boiling in twenty times its quantity of water, was quite as globular under the microscope as previously. With a smaller proportion of water, the mixture became very turbid, sometimes clots were formed in a pearl liquid, in which a fine sediment took place, which appeared much more globular than the clots or curdy masses.

4. In general, water in which pus has been agitated remains somewhat milky, with an abundant close white sediment; but after two, or three, or more ablutions, the water becomes clear on standing, and the sediment more curdy.

Sect. IV. *Agency of Alcohol of Wine.*

Action of alcohol on pus.

The different kinds of exsiccated pus exposed to the agency of this menstruum, and treated as described in a former

former paper, Phil. Trans. 1809, P. II, p. 329*, the results were similar, except in the proportion of products.

1. These exsiccated substances afforded to this menstruum a smaller proportion of potash, but as much animal oxide and muriate of soda, as mucous sputum.

2. The undissolved matter left after repeated digestions in this menstruum afforded the same substances, but in smaller proportions, as mucous sputum.

3. Equal bulks of fresh pus, and rectified spirit of wine, afford a much thicker and more milky liquor, with a closer sediment, than expectorated mucous matter.

Sect. V. *Agency of acetous Acid.*

The purulent matters mixed with this acid became curdy, and rendered it milky; but on standing, a close white sediment appeared, the liquid above being clear, except in the case of the viscid pus, which exhibited leafy and fibrous masses, as hath been described with mucous sputum.

Action of acetous acid on pus.

By repeated digestion of the different kinds of pus in this menstruum, I obtained the same results, except the proportions of acetite of potash, and muriate of soda being smaller, as related in a former paper on mucous expectorated matter. Phil. Trans. 1809, P. II, p. 336†.

Sect. VI. *Some Experiments with different Objects, especially to distinguish Pus and Mucus.*

1. In the agency of sulphuric, nitric, and muriatic acids, in sufficient quantity to dissolve and decompound the substances under inquiry, I could perceive no important difference between them. The purulent matters indeed required a much greater proportion completely to dissolve them, than the transparent sputum. Also the more opaque and dense the sputum, the greater the resistance to dissolution. Sulphuric acid produced black liquids like those containing charcoal, smelling strongly of muriatic acid; but on dilution with water, they became clear. No precipitation

Comparative experiments on pus and mucus, with mineral acids.

* Journal, Vol. XXV, p. 260.

† Journal, Vol. XXV, p 266.

...ccurred on dilution with water, and on saturation with ...ed alkalis, but a trifling sediment appeared, which ...olved on the addition of the above acids.

mineral acids diluted, and vegetable acids,

...The mineral acids diluted, or added in small proportion ...and the vegetable acids, coagulate variously pus and mu...us fluids. Some become merely milky fluids, others cu... fluids, others afford fibrous and leafy masses in a transparent liquor, and others give a uniform thick mass of curd. On standing the deposits are accordingly of various forms, and the liquors above of various appearances; but I could discover no constant characteristic property of the substances by these experiments, as some writers have asserted.

fixed alkalis and lime,

3. The solid fixed alkalis, or lime, mixed with expectorated mucus, occasion a stronger smell of ammonia than with pus; or than with muco-purulent sputum. Some use may be perhaps made of this easy experiment to judge of the nature of varieties of the fluids in question, particularly as far as depends on the proportion of ammonia; for sometimes it cannot be perceived by the smell on mixing alkalis, but can by muriatic acid giving white vapours.

strong solutions of alkalis,

Concentrated liquid alkalis, added to both pus and mucus, dissolve them to produce clear liquids, except small curdy parts and motes. These curdy parts and motes resist dissolution also for some time even in nitric acid, and seem to be self-coagulated lymph. They are in much greater proportion in pus than mucus. The addition of acids to these alkaline dissolutions occasions precipitations: but no differences, or not with sufficient uniformity to afford criteria, were observed according to the observations of other experimenters.

strong solutions of neutral salts,

4. Concentrated aqueous solutions of various neutral salts, *viz.* muriate of ammonia; nitrate of potash; muriate of soda; sulphate of soda, &c.; being mixed in due quantity with pus of the kinds under examination, produce viscidity, like ropy expectorated matter, thickening like jelly, and less opacity. These changes have, in the case of muriate of ammonia, been called coagulation by Mr. Hunter; but by agitation in cold water the matters are diffused, and on standing, the pus is precipitated in its original state. I call these effects of the neutral salts inspissation, seemingly occasioned by their attracting water from the pus; for no such

such change is produced if either the purulent matter, or solution of salts, be diluted; nor is it produced if the pus be previously coagulated by caloric: also the inspissated pus is coagulable by caloric as usual. No such inspissation is produced by these salts in mucous sputum, or in muco-purulent sputum, so that undoubtedly it is a criterion as discovered by Mr. Hunter in the case of muriate of ammonia, and with other neutral salts, as now manifested.

4. I endeavoured to find some easy tests for distinguishing pus from mucus; but I did not succeed with the tanning principle; gallic acid; supersulphate of alumina; nitrate of silver, and other metallic salts; and as already said, various acids. They all produced precipitation of these animal matters, but not with observable characteristic differences. and other tests.

5. To observe the state in which the matter of pus is secreted, I procured the assistance of Mr. Maynard, the present house-surgeon of St. George's hospital, and Mr. George Ewbank, who had been on many occasions essentially serviceable in my inquiries. Square pieces of goldbeater's skin were applied to various sore legs after carefully removing the matter already secreted. In five or ten minutes the square pieces being removed, they were found wet with a limpid fluid. In this state they were inspected by the microscope, by which numerous globules were seen. In ten minutes farther the liquid was no longer limpid but opaque, like pus, in which the usual spherical particles were seen with the microscope as juſt mentioned. State in which pus is secreted.

Supposing objections might be offered on account of the alteration of texture of the skin employed, square pieces of glass were also applied. The results were the same in both trials. The two gentlemen above named, as well as Dr. Richard Harrison, and other pupils, who happened to be present, all concurred in the observation, that the limpid matter became opaque, and that while limpid it was, like pus, full of spherical particles.

(To be concluded in our next.)

V.

V.

…ount of a New Gas, with a Reply to Mr. MURRAY'S *… Observations on Oximuriatic Gas. By Mr.* JOHN *I… Y.*

To MR. NICHOLSON.

SIR,

Mr Davy's theory opposed by Mr. Murray.

ABOUT six months since Mr. Murray undertook to oppose Mr. Davy's theory respecting oximuriatic gas, and to defend the old hypothesis, in which this substance is considered as a compound of oxigen and an unknown basis called muriatic acid, and common muriatic acid gas as a compound of the same basis and water.

His experiments.

Independent of his general reasoning, the only arguments advanced by this gentleman in support of his opinions were derived from his own experiments, undertaken expressly for the purpose. His first attempt to discover oxigen in oximuriatic gas was by trying the action of this substance on carbonic oxide; and he concluded, that it did not exert any when the mixture of the two gasses, previously dried, was exposed to the influence of light. He then endeavoured to prove, that the addition of hidrogen to the mixture induced action, and the formation of carbonic acid gas. He also attempted to show that oximuriatic gas, if supplied in sufficient quantity, is capable of affording oxigen to sulphur in sulphuretted hidrogen, and of converting it into sulphureous or sulphuric acid.

Objections.

To account for these supposed changes in consequence of the presence of hidrogen, he was obliged to imagine, in opposition to all experimental evidences, that the composition of muriatic acid gas is indefinite: that the unknown basis combines with different proportions of water, but always retains the appearance and the gaseous state of common muriatic acid gas, hitherto the only subject of experiment.

Having given this outline of Mr. Murray's mode of defence of the old hypothesis, I shall briefly state the facts I ventured to oppose to it.

Water present

It was first shown, that muriatic acid gas, and the sulphuretted

phuretted liquor of Dr. Thomson, alone resulted from the action of dry oximuriatic gas on dry sulphuretted hidrogen; and that the production of sulphuric acid in Mr. Murray's experiment was owing to his having admitted water.

in the experiment.

New compound.

My brother, Mr. Davy, next discovered the existence of a new gas made in the same way as the gas employed in Mr. Murray's first experiments, in which he says he obtained carbonic acid, and possessed of the property of converting carbonic oxide into carbonic acid, it being a compound of oximuriatic gas and oxigen.

Experiment to show that oximuriatic gas does not acidify carbonic oxide.

Lastly, it appeared, that due allowance being made for the difficulty of entirely excluding moisture, pure oximuriatic gas is not capable of converting carbonic oxide into carbonic acid, when inflamed with a mixture of this gas and hidrogen. Thus, when 10 measures of carbonic oxide were subjected to the action of oximuriatic gas inflamed by an electric spark with hidrogen, only two measures disappeared, 8 measures of carbonic oxide remaining unaltered. A result perfectly satisfactory, I conceived, considering the minute quantity of the gasses operated upon, not altogether amounting to half a cubic inch; and recollecting, that half a grain of water contains sufficient oxigen to convert about four cubic inches of carbonic oxide into carbonic acid.

Mr. Murray of a contrary opinion.

Mr. Murray is of a different opinion. He considers, in his last communication, the disappearance of two measures of carbonic oxide, a demonstration, that oximuriatic gas is a compound of an unknown basis and oxigen. In the same paper, which is published in your Journal for June, he has given an account of the repetition of his experiment on the mixed gasses, employing pure oximuriatic gas; and he has arrived at the conclusion, "that the production of carbonic acid is established beyond the possibility of doubt."

I shall state the manner in which he conducted the experiment, and the evidences which satisfied him of the production of carbonic acid.

His experiment in support of it.

He exposed to light a mixture consisting of one volume of carbonic oxide and of the same quantity of hidrogen with twice that quantity of oximuriatic gas. After 36 hours he added ammoniacal gas to complete saturation, and, finding that most of the carbonic oxide had disappeared, and

and that one of the ammoniacal salts formed had the property of effervescing with dilute nitric acid; he, without any additional proofs, drew the conclusion just mentioned, "that the production of carbonic acid in this experiment "was established beyond the possibility of doubt."

A new acid gas the cause of his mistake.

I have now to announce the existence of a new acid gas, which operated in Mr. Murray's experiment, without his knowledge of its presence, and was the cause of those phenomena, which he erroneously attributed to the formation of carbonic acid gas.

Mr. Murray's experiment repeated.

Repeating this gentleman's experiment on the exposure of the mixture of the three gasses to light, and detecting, after the addition of ammonia, no traces of carbonic oxide; and perceiving, as he stated, an effervescence of the ammoniacal salt formed with nitric acid; I was induced to repeat also his experiment on the exposure of a mixture of carbonic oxide and oximuriatic gas to light without hidrogen. In this instance I obtained the same result, a total condensation by ammonia without the slightest remains of carbonic oxide.

Presence of water suspected but not to be found.

So satisfactory were the details of Mr. Murray's experiment, the result of which was asserted to be, "that dry carbonic oxide gas and oximuriatic gas do not act on each other;" that at first I could hardly believe, but that water was somewhere concealed in the apparatus, and I gave myself much trouble to discover its source, but in vain.

The gas examined.

The next step I took was to examine the gas, that resulted from the now evident action of oximuriatic gas on carbonic oxide. Mr. Brande was present at the time.

Its properties.

Finding that it did not fume when thrown into the atmosphere, that it had a most intolerable suffocating odour, that it was colourless, that it did not act on the mercury, and that water absorbed it very slowly, we immediately perceived, that it was a new and peculiar compound of carbonic oxide and oximuriatic gas, and this conclusion is fully confirmed by the investigation I have made of its properties.

I shall now mention only the most striking circumstances respecting it. It is my intention to give a full account of the experiments I have made on it, in a paper which I shall soon do myself the honour of offering to the Royal Society.

I have

I have found, that it is produced in two or three minutes when a mixture of equal volumes of carbonic oxide and oximuriatic gas is exposed in a tube over dry mercury to bright sunshine; and that the condensation, that takes place in their union, is exactly equal to one volume, so that this is the heaviest gas known excepting silicated fluoric acid gas. I have also ascertained, that it may be at any time formed without the direct rays of the sun—Light alone being necessary. Its acid character is well defined. It reddens litmus and combines with ammonia; and its saturating power is so great, that it condenses four times its volume of this gas, forming a perfectly neutral salt, deliquescent, and of course very soluble in water; and its attraction for the dry volatile alkali is so strong, that it decomposes carbonate of ammonia, and is not expelled by acetic acid from this alkali. The decomposition of this ammoniacal salt with effervescence by dilute nitric acid deceived Mr. Murray. Water in this instance is decomposed, its hidrogen is abstracted by the oximuriatic acid to form muriatic acid, and its oxigen by the carbonic oxide to produce carbonic acid, which is disengaged. This will appear evident, when it is known, that the new gas neither inflames on the passage of the electric spark with either oxigen or hidrogen alone, but that it detonates violently with a mixture of oxigen and hidrogen in proper proportions, and affords only muriatic and carbonic acid gas. The action too of several metals and their oxides on this gas is perfectly consistent with, indeed is quite demonstrative of its being a compound of equal volumes of carbonic oxide and oximuriatic gas, so condensed as to occupy half the space of the mixture of the two. Thus tin, zinc, and antimony, respectively heated in it in small bent glass tubes over mercury rapidly decompose it. In each instance carbonic oxide, exactly equal to the volume of the gas decomposed, is liberated, and a compound of the metal employed and oximuriatic gas is produced, the same precisely as is formed by the combustion of the metal in oximuriatic gas. The decomposition too is just as readily effected by the oxides of zinc and antimony; with the first carbonic acid gas is obtained, and a compound of zinc and oximuriatic gas;

Its production.

Its characters as an acid.

Decomposition of its ammoniacal salt.

Other properties of it.

Action of metals on it.

but with the last, the fusible protoxide being used, r of antimony is produced, and carbonic oxide libe- , and an infusible peroxide formed, a proof, if any was red, of the formation of carbonic acid in the preceding nce being owing to the decomposition of the oxide of zinc, and not of the oximuriatic gas.

It is composed of two acidifying principles united to one base.

These are some of the principal circumstances I have discovered respecting this new gas; a gas, which, as it reddens litmus and expels acids from ammonia in consequence of superior attraction, has every claim to be considered as a peculiar acid singularly composed of two acidifying principles united to one inflammable base.

After the preceding statement of facts, Mr. Murray, I should conceive, will be induced to renounce his conclusion, "that the production of carbonic acid in his experiment was established beyond the possibility of doubt;" and admit, that what he considered as carbonic acid was actually the new gas just described; and I should likewise imagine, that this gentleman in future will be more cautious in his assertions, and criticisms on the labours of others. Let the intelligent candid reader judge of the propriety of the following observation. Mr. Murray says, having previously stated, that he had found carbonic acid in all his experiments, "that the Messrs. Davys did not obtain it in theirs, because they did not look for it with sufficient care, or were not sufficiently aware of the fallacies, by which its production might be concealed." His considering the new gas as carbonic acid is another instance of the evil tendency of attachment to hypothesis. How just is the remark of Lord Bacon! quod mavult homo esse verum, id facile credit.

No carbonic acid formed from the combustion of dry carburetted hidrogen and oximuriatic gas.

In a former communication I have observed, that no carbonic acid appeared to be formed, when dry carburetted hidrogen and oximuriatic gas are inflamed by the electric spark, assigning as a reason for this belief, the precipitation of charcoal. I tried both olefiant gas and carburetted hidrogen procured by the decomposition of acetate of potash by heat. Mr. Murray says, that he has repeated the experiment, and that in this too I was deceived. Mr. Murray employed the gas produced by heat from moistened charcoal.

charcoal. It is surprising that he is not aware, that Dr. Henry found this gas to be a mixture of carburetted hidrogen and carbonic oxide; and that the formation of carbonic acid might be expected on passing a mixture of it and oximuriatic gas frequently through lime water, as he experienced, this being the result when a mixture of pure carbonic oxide and oximuriatic gas is thus treated.

There is nothing farther in Mr. Murray's communications, that requires notice, excepting a misuse of names. He sometimes writes properly, calling me "Mr. J. Davy," at other times, improperly "Mr. Davy," thus creating confusion, and rendering it impossible to distinguish opinions and statements which belong to me, and for which I alone am answerable, from those of Mr. Davy, my brother.

I am, Sir,

Your obedient humble servant,

London, August the 9th, JOHN DAVY.
1811.

VI.

Method of preparing a beautiful and permanent White for Water Colours. In a Letter from Mr. GROVER KEMP.

To MR. NICHOLSON.

SIR,

It is our duty to communicate all we can to the public.

IT is a very just remark of the ingenious and candid Chaptal, that, "at a time when the minds of all men are bent on confirming public happiness, every citizen owes to his country all the services that his situation allows him to accomplish; he should be eager to pay to society the tribute of those talents with which Heaven has favoured him; and there is no one who is not able to bring some materials to the foot of that superb edifice, which a virtuous government is erecting to the happiness of all *:" and believing with this

* See "Elémens de Chimie par M. Chaptal," Montpellier edition, 1790. Advertisement, p. 5.

...ed chemist, that it is the duty of every one to do ... can towards the advancement of general science, ... duced to lay before the public, through the highly respectable medium of the Philosophical Journal, a new and easy method of preparing a beautiful permanent white for water colours, calculated to stand the test of time; which, I understand, is at present a great desideratum among our artists. This being the case, I entertain a confident hope, that the present discovery may prove eminently useful.

Mentioned by Mr. Hume.

Through the information of a chemist of the name of Hume, the public is already in possession of the facts, that a colour can be prepared from barytes; and that this earth will furnish the only white for water painting, that never changes; which may also be mixed with any other colour without injury; but of its mode of preparation we have hitherto, I believe, remained entirely ignorant. This beautiful pigment, which not only surpasses in opacity and whiteness every thing of the kind I have ever met with, but possesses the peculiar advantage of being permanent, is prepared by the following simple process: Dissolve pure barytes, or the common native carbonate, in diluted nitro-muriatic acid; filter the solution, and add thereto as much carbonate of ammonia, previously dissolved in distilled water, as is sufficient to precipitate the earth; which may be separated by filtration, and, after repeated washings with distilled water, must be gradually dried by the heat of the sun, or a fire, and rubbed into a very fine powder, or made up into cakes for use. I decomposed some nitro-muriate of barytes with a solution of pure ammonia, but the precipitate was very inferior in colour to the above. An artist of acknowledged celebrity, who has used this white, speaks very encouragingly of it.

I remain, respectfully,

GROVER KEMP.

Brighton, 8th mo. 4th,
1811.

VIII.

VII.

The Natural History of Clouds. *By* LUKE HOWARD, *Esq.**

A CLOUD is a visible aggregate of minute drops of water suspended in the atmosphere.

The word is probably derived from the Anglo-Saxon ceh-loδ, *covered*, *hidden*, the face of heaven being so in those parts where clouds appear. The same aggregate, which in this situation is called cloud, obtains the name of mist, when seen to arise from the earth or waters; and fog, when it envelopes and covers the observer. Yet the two latter, viewed from a greater distance or elevation, present all the appearances of clouds; while these, in their turn, become mists and fogs, in proportion as we approach and penetrate them. It may be proper, therefore, for the sake of precision, that the term cloud, in philosophical language, should be made a general one, comprehending all such aggregates, however situate. Etymology and definition of the term cloud.

It is concluded, from numerous observations, that the particles of which a cloud consists are always more or less electrified. The hypothesis, which assumes the existence of vesicular vapour, and makes the particles of clouds to be hollow spheres, which unite and descend in rain when ruptured, however sanctioned by the authority of several eminent philosophers, does not seem necessary to the science of meteorology in its present state; it being evident, that the buoyancy of the particles is not more perfect than it ought to be, if we regard them as mere drops of water. In fact they always descend, and the water is elevated again only by being converted into invisible vapour. Formed of drops of water.

Natural History of Clouds.

Since the general introduction of accurate instruments for determining the changes of density, temperature, humidity, Prognostications of weather from them.

* This valuable paper was first inserted in Mr. Tilloch's Philosophical Magazine, and reprinted, with the author's revisions, in Dr. Rees's New Cyclopedia, article CLOUD, from which I have copied it, in order that the readers of our Journal may more completely understand the Meteorological Tables, which will in future appear in our work. W. N.

and electricity, which continually occur in the atmosphere, our knowledge of its constitution and properties has been considerably advanced. It is nevertheless true, that the philosopher of the present day is not more weather-wise than his predecessors in ancient times. He is still obliged to yield the palm in the science of prognostics to the shepherd, the ploughman, or the mariner; who, without troubling his head about the reasons of things, has learned, by tradition and experience, to connect certain appearances of the sky with certain approaching changes; of which those appearances are, in fact, a commencement or continuation, discoverable while the cause is yet at a distance. Undoubtedly the union of these two kinds of knowledge would best deserve to be entitled the science of meteorology; and it must tend, equally with the invention or perfection of philosophical instruments, to the improvement of this science, could we restore to its place the ancient and popular branch of it, now too much neglected by philosophers, which is founded wholly on natural phenomena. If we except the changes of the wind, some indications of moisture and dryness, and a few others of less importance, the whole of these may be traced to one common origin in the product resulting from the decomposition of vapour; which remains, during a certain interval, in a state of simple diffusion or suspension in the atmosphere. To give to the extensive collection of facts, which it is easy to make on this subject, a communicable and useful form; to render that attainable in a short time, which has been hitherto the exclusive treasure of the adepts of long experience, is the object of the writer of the following systematic nomenclature and natural history of clouds.

Modifications of Clouds.

Clouds are susceptible of various modifications.

By this term is intended the structure or manner of aggregation, in which the influence of certain constant laws is sufficiently evident amidst the infinite less diversities resulting from occasional causes.

Hence the principal modifications are as distinguishable from each other, as a tree from a hill, or the latter from a lake; although clouds, in the same modification, compared with

with each other, have often only the common resemblances which exist among trees, hills, and lakes, taken generally.

There are three simple and distinct modifications, which are thus named and defined.

1. Cirrus. *Def.* Nubes cirriformis tenuissima, quæ undique crescat. Cirrus.

The Cirrus. A cloud resembling a lock of hair, or a feather. Parallel flexuous, or diverging fibres, unlimited in the direction of their increase.

2. Cumulus. *Def.* Nubes densa cumulata, sursum crescens. Cumulus.

The Cumulus. A cloud which increases from above in dense, convex, or conical heaps.

3. Stratus. *Def.* Nubes strata, aquæ modo expansa, deorsum crescens. Stratus.

The Stratus. An extended, continuous, level sheet of cloud, increasing from beneath.

There are two modifications, which appear to be of an intermediate nature; these are:

4. Cirro-cumulus. *Def.* Nubeculæ subrotundæ connexæ vel ordinatè positæ. Cirro-cumulus.

The Cirro-Cumulus. A connected system of small roundish clouds, placed in close order, or contact.

5. Cirro-stratus. *Def.* Nubes extenuata, sub-concava vel undulata. Nubeculæ hujusmodi appositæ. Cirro-stratus.

The Cirro-stratus. A horizontal or slightly inclined sheet, attenuated at its circumference, concave downward, or undulated. Groups or patches having these characters.

Lastly, there are two modifications, which exhibit a compound structure, *viz.*

6. Cumulo-stratus. *Def.* Nubes densa, quæ basi cumuli structuram patentem cirro-strati, vel cirro-cumuli superdat. Cumulo-stratus.

The Cumulo-stratus. A cloud in which the structure of the cumulus is mixed with that of the cirro-stratus, or cirro-cumulus. The cumulus flattened at top, and overhanging its base.

7. Nimbus. *Def.* Nubes densa, supra patens et cirriformis, infra in pluviam abiens. Nimbus.

The

: Nimbus. A dense cloud, spreading out into a crown
…us, and passing beneath into a shower.

Of the Cirrus.

Cirrus described.

This is always the least dense, and commonly the most elevated modification. It is sometimes spread horizontally through a vast extent of atmosphere; the whole breadth of the sky being insufficient to show where it terminates. In this case, its parallel bars appear, by an optical deception, to converge in opposite points of the horizon. At others, it is exhibited in unconnected perpendicular bundles, of the most minute size. Between these extremes, it may be traced in every degree of extent and inclination to the horizon. In a serene sky the cirrus is first indicated by a few threads, pencilled in white, on the azure ground. Its increase takes place in various ways, and may be compared sometimes to vegetation, more often to crystallization. Thus, 1. Parallel threads are added to each other horizontally, and occasionally other strata of the same, crossing the first at right or oblique angles, until a delicate transparent veil is formed. 2. Parallel threads are collected into distinct groups, lying at various angles with the horizon. 3. Flexuous and diverging fibres are extended from the original stem, forming the resemblance of crests of feathers, locks of hair, &c. 4. The first formed threads, become, as it were, the supports from which others obliquely ascend or descend into the atmosphere. Lastly, A dense nucleus is sometimes formed, and short fibres shoot out from it in all directions.

Its formation.

…ery lofty.

The great elevation of the cirrus has been ascertained by geometrical observations. "The small white streaks of condensed vapour, which appear on the face of the sky, I have found," says Dalton, "by several careful observations, to be from three to five miles above the Earth's surface."

Viewed from the summits of the highest mountains, they appear as distant as from the plains. A more easy and not less convincing proof of their elevation may be deduced from their continuing to be tinged by the sun's rays in the evening twilight with the more vivid colours of the prism, while the denser clouds, having already passed through the same gradation, are in the deepest shade.

The

The duration of this cloud varies according to its staton in the atmosphere, and the presence or absence of other clouds: it is long, extending sometimes to thirty-six hours, when it appears alone, and at its greatest elevation; but shorter, or even very transient, when formed lower, and in the vicinity of the cumulus. Its duration.

By an inexperienced observer the cirrus would be pronounced absolutely motionless. On comparison with a fixed object, however, it is sometimes found to have a considerable progressive motion. The propagation of the cirrus, and the variable directions of its flexures, merit attentive observation, as being intimately connected with the variations of the wind, although undoubtedly not produced by the mere motion of the air. Its motion. Its connexion with the wind.

The general principles, which the imperfect notice hitherto bestowed on it seems to point out, are the following:

1. Its appearance is a general indication of wind; and it is most conspicuous and abundant before storms. Indications from it.

2. It is often a leeward cloud; or, when a group of cirri appears on the horizon, it seems to invite a current towards it: and the wind very often shifts into that quarter towards which the points are directed.

3. Horizontal sheets of the cirrus, more particularly those which carry streamers pointing upward, are among the indications of rain approaching, while the fringe-like depending ones are found to precede fair weather.

Of the Cumulus.

Clouds in this modification are commonly of dense structure. They are formed in the lower atmosphere; and move with the wind, or more properly with that current which flows next the Earth. The phenomena of the cumulus are usually these: In the latter part of a clear morning, a small irregular spot appears suddenly at a moderate elevation. This is the *nucleus*, or commencement of the cloud, the upper part of which soon becomes convex and well defined, while the lower continues irregularly plane. On the convex surface the increase visibly takes place, one heap or protuberance succeeding another, and again losing itself in a subsequent one, until a pile of cloud of an irregular hemispherical Cumulus described. Its formation.

hemispherical form is raised; which floats along, presenting its apex to the zenith, while the base, or rather the lower surface of the baseless fabric, continues parallel to the horizon.

When these clouds are of considerable magnitude, they remain at proportionably great distances. When smaller, they croud the sky by a nearer approach to each other. In each case the bases range in the same plane; and the increase of each keeps pace with that of its neighbour, the intervening space remaining clear.

Decrease. The cumulus often arrives at its greatest magnitude early in the afternoon, when the temperature of the day is at its maximum. As the sun declines, it gradually decreases, retaining its character till towards sun-set, when it is more or less hastily broken up, and evaporates, leaving the sky clear, as in the early part of the morning. Its tints are often vivid, and pass through the most pleasing gradation during this last hour of its existence.

Indication. The preceding phenomena form the history of the pure cumulus, as it may be termed, when no other modification appears along with it. They are both the accompaniments and prognostics of the fairest weather.

Of the Stratus.

Stratus described. The stratus has a moderate degree of density. It is the lowest of the modifications, being formed in contact with the earth or water. It comprehends those level creeping mists, which, in calm evenings, spread like an inundation from the valleys, lakes, and rivers, to the higher ground.

Unlike the cumulus, which belongs to the day, and rarely survives the setting sun, this cloud accompanies the shades of night, and commonly vanishes before the ascending luminary. The evaporation commences from below. At the moment of the separation of the stratus from the Earth, its character is changed, and it puts on the appearance of the nascent cumulus.

Indication. The nocturnal visits of the stratus have been always held a presage of fair weather. Thus Virgil:

> "At nebulæ magis ima petunt, campoque recumbunt."
>
> Then mists the hills forsake and shroud the plain.

The

The meteorological axioms of this great poet were probably selected from the popular ones of his age, as confirmed by his own experience. Hence they ever agree with that of his readers. There are few days in the whole year more calm and serene than those the morning of which break out through the stratus. They are the halcyon days of our autumn: an interval of repose between the equinoctial gales and the storms of winter.

Of the Cirro-cumulus.

The intermediate nature of this cloud may be ascertained by tracing its origin, as well as inferred from its structure. The cirrus, in its slow descent through the air, may be seen to pass into this and the next modification; although its previous appearance does not seem absolutely necessary to the production of either. Cirro-cumulus described.

Most of our readers will recollect the appearance of the icy efflorescences on the panes of windows, gradually melting into an assemblage of drops, which adhere to the glass, retaining somewhat of the same figure, deprived of its right lines and angles. Such is the change of form which the cirrus undergoes, in passing to the state of the cirro-cumulus. And, as the water on the windows is occasionally converted again into spiculæ of ice, so these small rounded masses sometimes suddenly resume the forms of the cirrus. In the oblique denser tufts of the latter, the change to the spheroidal form often begins at one extremity, and proceeds gradually to the other, during which the cloud resembles a ball of flax, with an end left unwound and flying out. All the cirri in the same group, and frequently all those in view, observe the same law in these changes.

The cirro-cumulus forms a very beautiful sky. Numerous distinct beds are sometimes seen floating at different altitudes, which appear to consist of smaller and still smaller clouds, as the eye traces them into the blue expanse. It is most frequent in summer; is the natural harbinger of increased temperature; and, consequently, one of the best indications of fair weather, when permanent or frequently repeated. A more transient display of it is, however, frequent in the interval of warm showers, and in winter. There Indications.

are

are also certain forms of it, more deep and dense than ordinary, and arranged on a curved base, which enter into the peculiar features of thunder-storm.

It is usually found to accord with a rising barometer.

Of the Cirro-stratus.

Cirro stratus described.

This is a multiform cloud, and can only be detected in its various appearances by an attention to its distinctive characters. It is always an attenuated sheet, or patch, floating on the air, in a position nearly or quite horizontal. As we have compared the cirrus to dry flax, we may here consider it as drenched in water, and having its spreading fibres reduced to a closer and recumbent form. Viewed over head, it is remarkable for its uniform hazy continuity, and in the horizon for its great appearance of density, the consequence of its being seen edgewise. In this situation, also, it sometimes cuts the sun's or moon's disk across with a dark line; of which Virgil,

Indications.

"Ille ubi nascentem maculis variaverit ortum
Conditus in nubem, medioque refugerit orbe,
Suspecti tibi sint imbres; namque urget ab alto
Arboribusque, satisque notus, pecorique sinister."

Georgic, lib. i.

Or should his rising orb distorted shine
Through spots, or fast behind a cloud's dark line
Retire eclipsed; then let the swain prepare
For rainy torrents: a tempestuous air,
Swift from the southern deep, comes fraught with ill,
The corn and fruits to waste, the flocks to chill.

The cirro-stratus is the natural indication of depression of temperature, wind, and rain. In order to make a proper use of it in this respect, it is necessary to attend to the time of its appearance, to its continuance, and its accompaniments. This cloud sometimes alternates with the cirro-cumulus, either at different intervals of the day, or in the same sky, or even in the same stratum, which may consequently be seen successively in each modification, and at interva's, partly in one, partly in the other. In this case the prognostic is doubtful,

doubtful, and regard is to be had to that which ultimately prevails.

Again, there is a transient appearance of the cirro-stratus, which often accompanies the production of dew in the evening, and denotes an atmosphere but lightly surcharged with vapour. Not so when it appears earlier in the day, or at sun-rise (according to the preceding quotation), and attended with the rudiments of the cumulus. In general, the weather may be suspected of a strong tendency to wind and rain, as often as the sky is both hazy, and deformed with numerous small patches of cloud, in which the extenuated character predominates; and these appearances, together with an abundance of cirro-cumulus, indicate thunder. Before storms of wind, there is in particular a feature of cirro-stratus, often very slightly expressed, and in one quarter only, which resembles the architectural cyma.

But the most formidable appearance of the cirro-stratus is that of extensive sheets, descending from the highest regions of the atmosphere, and scarcely discernible for a time, but by the prismatic colours which they assume in the vicinity of the sun's or moon's place. These are the skreens on which are described the immense circles of haloes, forming, by their occasional intersections, parhelia, and paraselenia, mock suns and moons, which sometimes vie in splendour with the luminaries themselves. It is easy for those who are acquainted with the principles of optics, to conceive how these intersecting circles are produced by light passing through sheets of cloud placed at different heights and angles.

Haloes, parhelia, &c.

Consistent with this is the prognostic of foul weather commonly deduced from the appearance of the halo. After a solar halo in spring, or the early part of summer, a series of wet and cold weather may be expected, although it should not commence for some days; during which, nevertheless, the same state of the atmosphere subsists, as is often manifest from the repetition of the halo. Those which surround the moon in clear nights indicate rain or snow, according to the season of the year.

In mountainous and even hilly countries, the cirro-stratus is frequently seen adhering to the more elevated points of land.

land. In winter it also visits the plains, in the form of a very wet and durable mist, the drops of which are nevertheless too small to be visible, and which, unlike the stratus, is more dense on rising grounds than in the valleys.

The cirro-stratus usually accords with a sinking state of the barometer.

Of the Cumulo-stratus.

Cumulo-stratus and its formation described.

The formation of the cirro-cumulus, or cirro-stratus, by condensed vapour, descending from the higher atmosphere, does not prevent the cumulus from being produced out of the water, which, in the mean time, evaporates from the Earth, and ascends to the middle region. In this case, the two modifications after a while come into contact, and present to the attentive observer a succession of curious appearances.

While the cumulus is rapidly increasing upward, a delicate fleece, of a structure visibly different, sometimes attaches itself to its summit, where it reposes as on a mountain. This fleece is a cirro-stratus; and the materials of which it is formed are brought by a superior current overtaking or meeting the cumulus. Frequently, the cumulus in its increase breaks through the cirro-stratus, and appears again above it, but with a visible change in the aggregation, which now becomes rocky, perpendicular, and, finally, overhanging. If the cirro-stratus ſhould itself increase too fast to be swallowed up by the cumulus, the latter after a while extends its protuberances laterally, and attaches itself by them to the superior mass of cloud.

When the cirro-cumulus, in like manner, occupies the superior place, a cumulus rising beneath it is susceptible of the same union by mutual attraction; the result of which, as in the former case, is a large, lofty, and dense cloud, which often subsists through the day; and in the evening undergoes the usual evaporation.

It is not, however, absolutely necessary to the production of this cloud, that either of the superior modifications should be previously formed. In a favourable state of the atmosphere, the cumulus itself, after having arrived at a certain magnitude, suddenly begins to overgrow its base, and produces

duces a cloud, which, in regard to both its form and its rapid growth, may be compared to a mushroom.

The cumulo-stratus usually prevails in the completely overcast sky. In this it presents appearances not easy to be described, but which may be classed by a due attention to the theory of this cloud. At present it is intended to comprehend under it every mode of union between different ſtrata, which is not productive of rain. Future investigation may point out distinctions, which at present we are not prepared to make.

This modification is most frequent during a mean elevation of the barometer, or that which is denominated *changeable*, when the wind blows from the west, with occasional deviations towards the north and south. In respect to temperature, it has a wide range, and may usher in a fall of snow, as well as a thunder-storm. Of the latter, indeed, it is among the regular harbingers, but with peculiar appearances. During the suffocating calm which prevails before the first discharge of the atmospheric electricity, it may be seen in different points of the horizon, rapidly swelling to a stupendous magnitude, most curiously wreathed and curled, "fretted and embossed" in its substance, and flanked at different heights by the delicate opake streaks of the cirro-stratus. The whole presents a spectacle of peculiar magnificence, in contemplating which one may imagine an invisible agent collecting in this immense laboratory the energies of the storm, and arranging innumerable batteries for the subsequent explosions. Indications.

It will appear by what we have already stated, that the cumulo-stratus affords in general a doubtful prognostic. When it is formed in the morning, the day often proves fair, though overcast; and if the cirro-stratus has contributed to its formation, there will probably ensue heavy showers on the second or third day. When it subsists a long time, the character of its superior spreading part may be consulted, which, if it be decidedly either that of the cirro-stratus, or cirro-cumulus, the usual result of their appearance may be expected.

Of the Nimbus.

To have a correct notion of this cloud, the reader has only Nimbus de-

to

scribed. to take the opportunity of examining a shower in profile as it approaches from the horizon. He will see the dense gloom, which experience teaches him to regard as a mass of descending rain, losing itself above in a cloud, which commonly spreads in one continuous sheet to a great distance all around the shower; insomuch that while the latter is on the horizon at several miles distance, the edge of the cloud has frequently arrived in the zenith. He will perceive, that this spreading crown of the shower advances regularly before it, and that, whether viewed from a distance or over-head, it exhibits in a greater or less degree the fibrous structure of the cirrus. After the shower has passed over, he will commonly observe the same appearances in the part of the cloud which follows it; and in squally weather he will sometimes be able to repeat these observations on many different showers appearing successively; or at the same time, in different quarters. The term *nimbus* is intended strictly to denote no more than this inverted cone of cloud, from which a sudden or dense local shower, whether of rain, snow, or hail, for the difference is not essential in either case, is seen to descend. As it rises to a great height in the atmosphere, it may be seen from a distance of many miles; and so constant is the result of a shower arriving with it, that though, in a few instances, perhaps from the small quantity of the rain, we have not been able to discover the usual obscurity beneath it, while at a distance, we believe it may be laid down as a general rule, on as good grounds as in most other cases, that rain, snow, or hail, is falling on the tract over which it is spread.

> "Qualis ubi ad terras abrupto sidere *nimbus*
> It mare per medium, miseris heu prescia longé
> Horrescunt corda agricolis." Virgil.

> So while far off at sea the storm-cloud lowers,
> And on the darken'd wave its fury pours,
> Mid crops unreap'd the hapless peasants stand,
> And shuddering view its rapid course to land.

There is a great difference, at different times, in the proportion which the inverted cone of cloud bears to the column of rain, &c., in which it terminates; and in a very turbid and moist

moist atmosphere, the character of the upper part often approaches more nearly to the cirro-stratus than the cirrus. The more perfectly distinct and local the shower, and the clearer the rest of the air from other clouds, the more perfect the crown of cirrus, which, indeed, sometimes assumes an almost geometrical precision in its form and internal structure; the threads of the cirrus tending from all sides directly towards the top of the column.

Increased by cumuli:

The pure nimbus commonly moves with the wind, and from the rapidity of its passage affords but little to the rain-gauge. But it often happens, that it is formed in the midst of cumuli, which have already arrived at a great size. In this case the latter may be seen to enter successively into the focus at the top of the column, whence they never emerge; being visibly converted to the purpose of supplying materials for the irrigation, which thus becomes more abundant; and the shower is also occasionally thus propagated in a direction opposite to the wind.

Changes.

The nimbus, moreover, does not always originate in a cirrus. The cumulus, and more often the cumulo-stratus, may be seen to expand at their summit into a cirrose sheet, while the lower part is resolved into rain. On the contrary, the rain suddenly ceasing, and the nimbus remaining entire, the sharp extremities of the crown often retire into it; the sides assume the swelling folds, and the character is exchanged for that of cumulo-stratus. When the shower has expanded itself, and the sheets break, the superior portions usually turn to the cirro-cumulus or cirro-stratus, and the lower to the cumulus. When a total evaporation of the remaining cloud follows a shower, it is a very favourable prognostic. A nimbus is frequently accompanied by a cirro-stratus or two lying near it, and on a level with the densest part of the cloud. The nimbus of thunder-storms has many of these, as before observed of the cumulo-stratus, arranged at different heights; which, with the grotesque form of each cloud, and the hazy state of the medium, are sufficiently characteristic of the high electric state of the air at such times, and want only an attentive perusal (in nature) to enable the observer to ascertain it on future occasions. It appears, that the cumulo-stratus passes to the nimbus by a

Indications.

sudden

sudden change in its electricity: for in tracing the progress of a thunder-storm, through a long range of these clouds in the horizon, we have been satisfied, that the clouds, which had ceased to afford explosive discharges, had undergone this change in their superior part, and were pouring down rain; while others, among which the lightning still played, or which were situate beyond it, retained their swelling and rounded forms some time longer.

Of the Origin, Suspension, and Destruction of Clouds.

Origin of clouds.

These aggregates consist of water, raised by evaporation, and become visible by condensation in the atmosphere.

Evaporation.

Respecting evaporation, and the state in which vapour subsists, there has been much diversity of opinion: and, of the several theories proposed, there is not one comprehensive enough to merit exclusive adoption. A number of general principles, however, have been established; which we shall employ, with the aid of those of electricity (hitherto not enough considered in its silent and gradual effects), to explain, though in an imperfect manner, the principal phenomena of clouds.

Vapour.

Evaporation consists in the union of water with caloric, and the escape of the compound as an invisible fluid, which we shall exclusively denominate *vapour*.

The air has no solvent action on it.

The solvent action of the air, to which this effect has been attributed by chemical philosophers in general, has been proved by comparative experiments on the force of vapour in air, and with air excluded, to have no perceptible share in it. The laws which govern the natural process, for these alone here interest us, may be thus briefly stated.

Laws of the natural process.

The force by which water is converted into vapour is directly as its temperature, other things being equal: but this force has to overcome an opposing one, of the same nature, inherent in the vapour which already exists in the atmosphere. For such vapour, by its elastic property, tends to exclude from the space it occupies every additional portion; and consequently to prevent the escape from the water of new vapour. Hence the temperatures being equal, the quantity of vapour produced will be less, the greater the quantity already diffused in the air.

But

But, though the *chemical* action of air is imperceptible, its *mechanical* effect is great. A moving atmosphere may double or triple the rate of evaporation, according to its velocity. For not only is the surface, from which only the vapour escapes, thus enlarged and changed; but the nascent vapour itself, which would otherwise hover a while upon it, to the obstruction of the process, is immediately brushed away and diffused.

Mechanical effect of air on evaporation.

By applying these principles, we may explain to ourselves various natural phenomena: as for instance; why the wind, after rain, becomes colder than even the rain which fell; being robbed of its caloric by the evaporation of the floating and deposited water, with which it is in contact: why snow sometimes totally disappears without melting, and the surface of ice becomes sensibly wasted and channelled; for these are warm, compared with the dry and frosty air which blows at such times, and consequently evaporate freely. In what manner, again, a strong westerly wind in summer or autumn brings up clouds, which on its cessation descend in rain: for it promotes evaporation by its mechanical effect, and the vapour escapes into an atmosphere already too moist to carry it off to any great distance. This will be evident by recurring to the principle before stated, that the vapour escapes by the force of the temperature of the water out of which it is formed; and, consequently, into a colder atmosphere it will still escape, though continually decomposed thereby.

Explanation of various phenomena.

Vapour is decomposed by air, in consequence of the superior affinity of the latter to caloric. This happens in two ways. 1. When vapour escapes or is propelled into air colder than itself; the result being a local dense cloud. 2. When a mixture of air and vapour is cooled; in which case there ensues a general turbiness, which we shall exclusively denominate *haze*. It is occasioned by minute floating particles of water; the caloric which, united to these, formed transparent vapour, having passed into the air.

Decomposition of vapour.

Haze.

Out of this haze clouds may be afterwards formed, by simple aggregation, or by electrical attraction. It abounds in the atmosphere during the most part of the year, occupying sometimes the higher, sometimes the lower, part thereof.

The quantity in which it exists may be judged of, at some periods, by the appearance of distant objects seen horizontally: at others, by the degree of intensity of the blue colour of the sky, which becomes paler by it, if indeed the blueness is not wholly due to this part of the medium.

Of the Nature of the Stratus.

Nature of the stratus.

This cloud is an example of the decomposition of vapour thrown into air of a lower temperature. The earth or water on which it reposes is always warmer than the cloud, as is also the clear air above. Thus, in a stratus, formed over a field with ponds, the temperature of the earth just below the turf was 57°; of the water, 59°; of the air, at an elevation of thirty feet, 55°; while that of the cloud, at four feet from the ground, was 49·5°. Hence this cloud preserves a level surface; and hence it uniformly vanishes, or begins to be driven upward, as soon as its temperature becomes equal to that of the earth. It is consequently due to the decomposition (in a small portion of the atmosphere) of the vapour which the earth and water continue to emit, after sunset, by the force of a temperature previously acquired. But the change in the lower air, which gives occasion to this local decomposition, is not so easily to be explained: for it appears that very often, in the evening of a clear day, the decrease of temperature in the atmosphere takes place in the same order in which the increase did in the morning: *viz.* beginning from the surface of the earth and proceeding upward. If the air never became colder, on these occasions, than the contiguous soil, the effect might very well be ascribed to the absorption of a quantity of caloric by the latter. But we see that, in the present instance, it became colder by seven degrees, though vapour was still decomposing: and this in a perfect calm, which, in a great degree, forbids another supposition, of the exchange of a quantity of heated air below, for as much cold air from the higher atmosphere, otherwise this would seem a sufficient account of the matter.

Clouds not so good conductors as supposed.

The electric charge of the stratus, which is always positive, and sometimes highly so, notwithstanding the contact of its lower surface with the earth, seems to prove, that a cloud

cloud is not even so good a conductor as has been supposed, and that the fluid, in certain cases, may be very gradually transmitted through it. Positive electricity being that proper to the atmosphere in fair weather, we should naturally expect to find it in this cloud.

It might be worth while to examine the air above, with a view to discover whether there exists in the latter a negative counter-charge. It will appear, from a consideration of the principles before stated, why this cloud is almost peculiar to the autumn. The gradual decline of the sun, at this season, keeps the atmosphere constantly surcharged with vapour, which is ultimately disposed of in rain; and hence follow gales of wind. The stratus, therefore, though an immediate indication and accompaniment of fair weather, affords an unfavourable prognostic in the early part of summer; as it shows that a tendency has already begun to extensive precipitation, at a time when the usual predominant feature is increasing dryness. Indications.

Of the Nature of the Cumulus.

The heating effect of the sun's rays on the atmosphere is greatest near the surface of the Earth, and diminishes gradually in ascending. The diminution proceeds in fair weather at the rate of about one degree for each hundred yards, as appears by observations with the thermometer on stations of known difference in altitude. Nature of the cumulus.

This inequality appears to give rise to the cumulus, on the same principles as those of the stratus, but the effects are more complicated. Vapour is generated, as before, at the surface of the Earth, but it is thrown into an atmosphere heated by the sun. Here it maintains its elastic state, and, in proportion to the supply from below, the whole quantity existing in the atmosphere is compelled to rise. In doing this, it changes its climate, and arrives among air of a lower temperature, where a portion is continually decomposed, filling the middle region with haze. Of this, small aggregates begin to be formed, the increase of which is at first determined by no particular law. But the aggregate is not in equilibrium with the air. It tends to subside, and in the

Nature of the cumulus.

mean time the increase of temperature is proceeding upward. Hence the lower part soon finds a position in a plane of air sufficiently warm to evaporate it: and as this effect is regulated, in general, by the elevation alone, we see these aggregates assume each a flat base, resting as it were on the same plane, parallel to the Earth's surface. The remainder of the cloud sports in all the varieties of the spheroid, and more rarely of the cone; according to the course of the showers of minute particles of water, which we may consider (though invisible in their progress) as descending upon it. The vapour generated at the base is, probably, in part condensed on the surface of the colder particles of the cloud above. While the supply from the haze exceeds the waste by evaporation, the cloud increases: when the latter has begun to prevail, it may be traced through various stages of diminution to its final wreck, on sinking wholly into the warmer atmosphere. This happens commonly about sunset; because the ascending current of vapour, the source of the phenomenon, then slackens or ceases; and the lower air parting with its redundant caloric to the higher, we unexpectedly see the dense clouds evaporate, at the very time when the chill of the evening is felt below, and the dew falls.

But it does not appear, that the causes we have hitherto enumerated are fully adequate to the phenomenon. The increase of the cumulus is often more rapid than consists with the notion of simple attraction, exercised between distant particles of water, in a resisting medium. When a cumulus is thus increasing, the small aggregates in its way do not usually join it, but seem to vanish before it. Lastly, the cumulus itself, however dense, never descends in rain. It is difficult to conceive, that so powerful an attraction could exist for many hours, without bringing the particles together into larger and larger drops, until they were too heavy for longer suspension. If we suppose, however, that, from the commencement of its aggregation, the cumulus becomes a positively electrified mass, these difficulties vanish. This mass may electrify negatively, and attract into itself, from great distances, both the dispersed particles of water and those which have already united in much smaller masses. Its

Its particles must be mutually repulsive, and cannot come into contact without a change of state: the same may be said of the respective clouds in this modification, when they do not differ too much in surface.

Of the Nature of the Cirro-stratus.

When a portion of the atmosphere, charged with vapour, is brought over a tract of land of lower temperature than itself, its caloric is abstracted in sufficient quantity, usually to occasion a decomposition of some of the vapour, and a consequent general turbidness. Nature of the cirro-stratus.

The sweating, as it is improperly called, of walls and pavements in a thaw, and when rain is about to come on, is from this cause; the vapour being decomposed on their surfaces. Dampness on walls &c. The mist which ensues at these times obscures distant objects, and occasions the trees, against which it is borne by the wind, to drip plentifully. Mist. It is in fact a cirro-stratus in contact with the Earth, and no phenomenon is more familiar to the inhabitants of hilly tracts. The same general depression of temperature may happen in another way, and higher in the atmosphere. When a cold and moist air flows over a warmer vaporous one, it is obvious, that the former may be warmed, and become more transparent, at the expense of the latter; which, from the same cause, must become turbid. The haze thus produced will not subside with the uniform motion of dew, but rather in sheets, becoming more dense as they descend, both from the approximation of their particles, and addition from the vapour they meet with. But the cirro-stratus is far from assuming always the simple form, to which the mere effects of gravity might be supposed to give rise. It exhibits changes, which can only be attributed to the acquisition, or passage through it, of such small portions of electricity, as in a humid medium we may conceive a cloud to be susceptible of. On these occasions it tends either to the state of cirrus, or that of cirro-cumulus, of which we shall treat presently.

The reason of the prognostic afforded by the cirro-stratus will now be evident. Indications. It gives us notice of a change in the state of the superior atmosphere, which we could not other-

wise

wise be certain of, until the current, in its course of propagation downward, had begun to affect the denser clouds, thrown up by the superficial evaporation. It is not very uncommon to see the cirro-stratus evidently brought by a wind, moving in a different direction from that wherein the cumuli are immersed on which it settles. In this case the latter are speedily arrested by it, and assume the new course, or descend in rain, by a change of their electricity.

Of the Nature of the Cirro-cumulus.

Nature of the cirro-cumulus. Let us now reverse the former case, and consider the upper current as both vaporized, and warmer than the air below.

It is probable, that the upper is then cooled by that part of the lower which is next to it, though very slowly, from the difficult transmission of caloric downward. The decomposition of the vapour in the upper current by this means may give origin to the cirro-cumulus; and the peculiar aggregation of this cloud, as distinguishable from that of the cirro-stratus, may be the result of its acquiring electricity in its descent in a much greater degree. Such, at least, is the inference we may deduce from its abundance before thunder storms; when it is occasionally seen to arrive with the wind in extensive flocks or strata, moving with unequal velocity, and by consequence overtaking each other, until they form a dense stationary mass.

Indications. This explanation of the origin of the cirro-cumulus is principally deduced from an observation, which we have now so often repeated, as to regard it as a meteorological axiom; *that the temperature of the day following, exceeds that of the day on which it appears.* Hence, when it continues to recur daily, the weather still grows warmer, until a thunder-storm, in some quarter of the heated tract, puts a period to the insulation of the clouds.

Of the Nature of the Cumulo-stratus.

In attempting to assign causes to phenomena so complicated, as those which this modification presents, we may be in

in danger of admitting a greater number than are really necessary. It is apparent, however, that in the state of things most favourable to the production of the cumulo-stratus, there exists a precipitation, independent of that which gives rise to the cumulus, and situate in a higher region. As this precipitation affords sometimes the cirro-cumulus, at others the cirro-stratus, we need not assign to it any other cause than the one already mentioned, *viz.* a superior vaporized current of air. It is not inconsistent with the principles we have laid down respecting the cumulus, that this cloud should also be produced at the same time; it being requisite only that there exist a sufficient action of the sun on the Earth's surface, or a sufficient temperature derived therefrom. The inosculation of these two orders of cloud, the singular union which follows, and the establishment of a new centre of attraction, towards which the whole future increase tends, is the prominent feature in this modification, and the chief fact which remains to be accounted for. As this effect is not constant and uniform, it cannot be ascribed to gravity alone. Reasoning from analogy, rather than from direct experiment, which it is not easy here to apply, we may attribute it to a difference in the electric charge of the respective clouds; which difference, though small, ought to produce the usual appearances of bodies charged plus and minus; *viz.* mutual approach and contact. This effect, however, appears to ensue rather with regard to the masses than to the individual particles.

Nature of the cumulo stratus.

The effect of the highly vaporized state of the higher atmosphere is often discernible in the cumulus from its earliest appearance; and it is easy to determine, at certain times, that this cloud, if it continue long, will pass to the present modification. The effect we mean to point out is the uneven growth of the cloud; numerous small masses attaching themselves to its surface, and giving it an appearance not unlike the curls of a fleece of wool; particularly when seen beneath the sun, in a situation where the projecting parts may catch the light. If we admit that the cumulus acts, as well by electrical attraction, as by that of gravity, on the surrounding materials, we may here consider them as arriv-

ing

Nature of the cumulo-stratus.

ing by subsidence in too great plenty to be immediately assimilated; in consequence of which they tend to unite among themselves. A still greater quantity of haze, in the region next above the cumulus, gives rise to the curious phenomenon of the *cloud-capped* cloud; when the cumulus is covered at its summit with a cirro-stratus; in the same manner as, in mountainous tracts, this cloud reposes on an elevated point of land. The cause is probably alike in each case, whether it be a lower temperature, or a diminished electricity, which determines to this particular spot the commencement of the aggregation of the cirro-stratus. We may next consider the cumulo-stratus perfectly formed, and endeavour to assign a cause for its occasional long continuance: which, however, exceeds the day of its formation only on the approach of thunder: this cloud, as well as the cumulus, very commonly vanishing about sun-set, and reappearing the next day, for some time. The two strata of the atmosphere, which form the superior and inferior boundaries of the cloud, are probably, during this time, in somewhat different states of electricity; the one also depositing water, the other receiving it; the broad surface of the cumulo-stratus may be regarded as a coating, applied to the upper stratum; and receiving from it a continual accession of charged particles of water, the electricity of which is slowly transmitted through the intermediate portion, down to the base of the cloud, which is often some hundred feet below; and where a continual evaporation counteracts the increase above. Here, while the mass continues in this modification, the progress of the electricity downwards is arrested by the dry air: for although the insulated rod is found sometimes to be affected with positive, sometimes with negative signs, while the base of such clouds is over it, this effect is commonly influential; and the rod is not charged, as by the passage of the nimbus. How the electricity of this cloud is affected by the constant evaporation of a portion at the base remains to be ascertained; and the same may be said as to the cumulus.

Of the Nature of the Cirrus.

Nature of the cirrus.

It was necessary to defer the consideration of the nature of this cloud, until we had developed, in a considerable degree, the principles on which our theory proceeds. The reader will have seen, that we assume the fact of the slow transmission of the electric fluid through clouds: which in this, as in a former instance, we apply rather analogically than by induction; the modification in question being usually so high in the atmosphere, that the electric state of the latter, above and below it, cannot easily be found by actual experiment. Proceeding, however, on this assumption, we suppose, that the cirrus resembles in its state a lock of hair, or a feather, insulated and charged; or rather, that its arrangements result from the same cause with those of the coloured powders, which electricians project on a cake of wax, after having touched it with the knob of a charged phial, and which fall into a variety of configurations on the surface. Thus the cirrus may be formed in the air out of such floating particles of water as are present, and may serve the purpose of collecting and transmitting the electric fluid. It is during the prevalence of variable winds, that the cirrus most abounds; and it is reasonable to conclude, that the portions of air, which at these seasons are transported from place to place, gliding over or intersecting each other, usually differ sufficiently in temperature to occasion a slight decomposition of the vapour of one of the currents, and in their electric charge sufficiently to induce a communication by means of the conducting medium so formed. Again, in the gradual cooling of a perfectly calm plate of air, situate at a great elevation, and consequently free from the occasional causes of disturbance which prevail below, it is not improbable that the separation of the caloric from the vapour, and the collection of the electrised water from the air, may go on together, by a process similar to the crystallization of salts, in which much caloric is liberated into the medium. This opinion, at least, seems to be advanced by Kirwan, in his "Essay on the Variations of the Atmosphere," and we may consider the vegetating cirrus as the proper example of it.

Another

Nature of the cirrus.

Another conjecture might yet be started as to the cirrus. It might be regarded as a cloud wholly formed of minute spiculæ of ice; since the air, at a certain elevation, is sufficiently cold throughout the year for this effect. But if it should be found, that the particles of clouds are susceptible of a rectilinear arrangement in any case at a temperature exceeding 32°, there would be no necessity for this supposition.

If the appearances of the cirrus are as frequent and various at sea as on land, it cannot be doubted, that intelligent mariners would find their account in keeping a register of them, as connected with the changes of wind, &c., making due allowance for the change of station in different observations when under sail.

The buoyancy of the cirrus seems to be most perfect during its first increase. It always follows, at length, the common course of gravity; and the change to the cirro-cumulus, or cirro-stratus, which certainly depends on the state of the medium it falls into, may be ascribed to the retention or loss of the electricity.

Of the Nature of the Nimbus.

Nature of the nimbus.

This phenomenon may be thought to be improperly denominated a modification of cloud, since it consists usually of a column of descending rain, snow, or hail, seen in connection with the cloud affording it. As the concluding link in the chain of atmospherical precipitation, it seems, nevertheless, most advantageously placed here; and its history, though far from including all that we may observe, and could wish to have explained, on the subject of rain, is more decidedly illustrative of the nature of clouds in general than that of any other modification. Moreover it is sometimes observed to be formed before the rain begins, which affords sufficient ground for considering it as a distinct modification of cloud. We owe to the bold and penetrating conjecture of Franklin, on the identity of lightning and the electric spark, the invention of a method of investigating the electricity of clouds: which, in the hands of experimentalists, has since brought out a mass of facts abundantly sufficient to establish that proposition; and which

also

also throws considerable light on the theory of rain, and other depositions from the atmosphere. By this method the structure of the nimbus may at any time, when it passes over us, be demonstrated to be that of a natural conductor, by which the positive charge of the higher atmosphere is brought down to the Earth. For this purpose, there is provided a rod of iron, or other metal, well insulated on a pillar of varnished glass, the latter being defended from rain by an inverted funnel, soldered or cemented to the part of the rod next above it. The rod should be furnished with several points of wire, a few inches long; and it need not be an elevated one for this purpose, provided the extremity is clear of other objects capable of drawing off the fluid. The charge is ascertained by pith balls of a larger or smaller diameter, to suit the occasion, suspended by flaxen threads, on a wire fixed into the lower part of the rod, and terminating in a ball. Near the latter it is proper to have another ball fixed on a stout wire, passing into the ground, to which the fluid, when abundant, may escape in sparks. This instrument exhibits a charge of the same kind with that of the air in which it is immersed; or, in case of rain, &c., the charge of the latter, as compared with that of the air. We will give, in the first place, the appearance which we have recently observed during the passage over the rod of a nimbus of the most simple structure, having neither a cumulus nor a cirro-stratus attached to it; which moved along with the lower current through the clear atmosphere, and discharged a shower of large opaque hail, the air below being very dry. During the approach of the cloud from the north-east, the pith-balls remained close until the spreading crown, which characterizes this modification, had arrived in the zenith. At this time, and while the shower itself was still three or four miles distant, they opened negative. As the cloud came nearer, their divergence increased, until it amounted to full two inches, at which time sparks of considerable strength might be drawn from the rod. After this the negative charge gradually went off, and the balls touched again. In a few moments the edge of the shower, mixed with a few drops of rain, arrived at the conductor, and the balls instantly opened positive, the charge gradually increasing

Its electrical state shown.

Phenomena described.

increasing until sparks were emitted more freely than before. This charge continued during the passage of the hail, and went off gradually as soon as it was clear of the instrument. After having closed, the balls opened again negative, and this charge increased to a considerable intensity, as the shower receded towards the south and south-west, after which it gradually went off: the balls closed, and finally were left slightly positive. From these facts, the reader, who is conversant in electricity, will deduce the structure of the lower part at least of the shower. He will see, that the descending hail formed a column positively electrified. This, which might be six or seven miles in diameter, was surrounded with a cylinder of negative electricity, probably extending in every direction three miles farther, and resulting from the action of the positive centre on the dry atmosphere, in which it was moving. Now the amount of the hail, when melted, was considerably less than $\frac{1}{100}$th of an inch in the rain gauge; and could the descent of the electric fluid through the whole space have been rendered as obvious to our senses as that of the hail, we should probably have said, that the shower consisted of fire more truly than of ice.

Whence the electricity?

The question that naturally presents itself is, Whence came this flood of electricity which accompanied the hail? It was not from the circumstance of the water being frozen, since a hard shower of rain equally exhibits a charge, but with this remarkable difference, that whereas snow, sleet, and hail, are always positive, rain is found sometimes positive, sometimes negative. The reader may consult, on this head, an extensive collection of facts in Read's Journal of Atmospherical Electricity, "Phil. Trans." Vol. LXXXII. The probable sources of negative rain will be presently mentioned; but to return to the question of the origin of the positive charge; if we attentively consider the structure of the nimbus, it is precisely that which, from the known properties of the electric fluid, we should propose for a conductor formed to acquire the latter. If we detach from it the falling column, and extraneous clouds which usually attend its progress, it will be found to consist of a close collection of fibres, diverging from the region of the cumulus, (where,

(where, it appears, the rapid union of the particles into drops is accomplished,) to a vast height and extent in the superior atmosphere. The conducting line, therefore, may be considered as prolonged from the top of the column to the very extremity of each of these fine fibres of cloud, which are often extended, in all directions, as correctly as those of a lock of hair insulated on a charged conductor. The intention in this case seems to be not so much the precipitation of water, as that of the electric fluid which keeps it in suspension. This purpose accomplished, (and the reader may conceive how great a discharge must be effected by a number of such machines acting at once on a small tract of country,) the water unites into larger drops through the whole extent of the atmosphere; it subsides in a continuous sheet, under which the condensed product of the superficial evaporation moves along, in the form denominated *scud*; and the rain comes down freely and generally, until the atmosphere is disburdened, or until the partial vacuum which is formed brings in a drier air from the northward.

Scud.

Negative, as well as nonelectric rain (which sometimes falls, though strong positive and negative signs precede or follow it in the clear air) must necessarily result from the action of a central mass of cloud, in which a strong positive charge exists, on the clouds of less extent which fall in its way; and it is to be considered also, that rain, at the elevation in which it is formed, may be perfectly nonelectric, (*i. e.* it may result from the union of clouds differing in electricity, and hence uniting in rain,) yet at the moment of arriving at the Earth it may differ so much in its charge from the atmosphere below, the only standard of comparison, as to be strongly negative or positive with respect to the latter. But these considerations belong more properly to the subject of atmospheric electricity.

Negative or nonelectric rain.

We shall conclude with a brief review of the modifications, ascending from the stratus, formed by the condensation of vapour, on its escape from the surface, to the cumulus, collecting the water arrested in the second stage of the ascent; both probably subsisting by virtue of a positive electricity. From these proceeding, through the partially conducting

Review of the modifications of cloud.

cumulo-

cumulo-stratus, to the cirro-stratus and cirro-cumulus; the latter positively charged, and considerably retentive of its charge; the former less perfectly insulated, and, perhaps, conducting horizontally; we arrive thus at the region, where the cirrus, light, elevated, and extended, obeys every impulse or invitation of that fluid, which, while it finds a conductor, ever operates in silence; but which, embodied and insulated in a denser collection of watery atoms, sooner or later bursts its barrier, leaps down in lightning, and glides through the nimbus from its elevated station to the Earth.

VIII.

Account of the Thunderstorms on the 19th of August. In a letter from THOMAS FORSTER, *Esq.*

To WM. NICHOLSON, Esq.

SIR,

Thunder-storms on the 19th.

I Wish to communicate to your meteorological readers some observations on the thunderstorms, that happened on the 19th inst., of which I shall request your insertion.

Weather on the 18th.

The 18th was warm, the maximum of the thermometer being about 73°. *Cumuli* prevailed during the day, but towards evening the *cirrus* appeared.

The first storm described.

Before 8 o'clock in the morning of the 19th, the sky was clouded. I observed two *strata*; the upper one appeared to be a uniform veil of cloud, while loose flocky cumuli floated beneath it; and in some places large masses seemed to be attracted towards it, and adhered to its surface, forming an unusual wavy sky, which increased in density. About half after eight I heard a single explosion, like the report of a large brass cannon; about twenty minutes after which two more such reports were heard, following each other in rapid succession, which were immediately succeeded by a long and loud peal of rolling thunder. The storm now came up very fast, in a direction nearly contrary to that of the current of wind below, with hard rain, and thunder and lightning.

Interval.

After the storm had subsided, *cumuli* were again seen sailing under

under a continuous sheet of cloud; some of them were loose flocculi, others large well defined masses. By degrees they became lost in the upper stratum; the sky became again very black, and thunder and lightning with rain again prevailed. During the process of the storm I heard, (beside the many peals of *rolling thunder*) another loud single explosion, which sounded like the hollow report of a mortar; it was preceded by a very vivid flash of lightning. I dwell particularly on this circumstance, because I have often noticed during storms two very dissimilar kinds of thunder. One is a long roll increasing in loudness while it continues; this is supposed by Mr. B. P. Van Mons to be caused by combustion of the two gasses of water*. The other is a loud and sharp explosion of short duration, and often a single report like that of a cannon; the lightning which precedes this is generally vivid and mischievous, it darts directly towards the Earth, or any other prominent object, as high trees, towers, &c., and is considered by Mr. B. P. Van Mons, as the flying off of electricity from an overcharged cloud†. I wish to direct the attention of meteorologists to the solution of this question. When mischief is done by lightning, is not the thunder which follows the flash generally of this latter kind?

Second storm:

Two dissimilar kinds of thunder.

The variations in the direction of the wind below, in stormy weather, as well as the contrary directions of the current above, constitute another curious object of philosophical speculation. Small air balloons might, in this case, become useful meteorological instruments. I have sent up a great many of them, and have generally seen them moved by several different currents of air.

Various currents of air in stormy weather.

Small air balloons as a meteorological instrument.

Yours &c.

Clapton, Hackney,
22d Aug. 1811.

THOMAS FORSTER.

* See Journal for October, 1809, Vol. XXIV, p. 106.

† The distinction of rain, into "rain of the *decomposition*," and "rain of the *recomposition*" of air, by Mr. Van Mons, has induced me to inquire; What is the electric state of rain with a rising, and what with a falling barometer?

IX.

13	W	29·91	29·79	29·85	
14	S W	29·83	29·76	29·795	
15	S	29·83	29·80	29·815	
16	S W	29·85	29·83	29·84	
17	S	29·85	29·75	29·80	
18	S W	——	——	——	
19	S E	29·94	29·75	29·845	
20	W	29·94	29·90	29·92	
21	Var.	29·88	29·82	29·85	
22	W	30·01	29·88	29·945	
23	N W	30·11	30·01	30·06	
24	N W	30·15	30·11	30·13	[illegible]
25	N W	30·14	30·12	30·13	[illegible]
26	S W	30·12	30·09	30·105	[illegible]
27	N	30·09	29·91	30·00	7
28	S E	29·91	29·85	29·88	7
29	N E	30·11	29·85	29 93	7
30	N	30·11	30·08	30·095	6
31	N E	30·08	——	——	6
AUG.					
1	N E	——	29·90	29·99	7
2	S	29·90	29·69	29·795	7
3	S W	29·67	29·58	29·625	7
4	N W	29·73	29·60	29·665	6
5	S	29·65	29·62	29·635	6
6	S	29·59	29·48	29·535	6
7	N W	29·60	29·50	29.55	6
8	S W	29·49	29 35	29·42	6
9	N W	29·60	29 48	29·54	6
10	N W	29·86	29·60	29·73	6
		30·15	29·35	29·835	7

[redacted]

NOTES.

July 15. Small rain about 2 p m. 19 A thunder shower early: fine day. 20, 21. Forty-eight hours rain. 22. Temperature 60°, (the *maximum* of the period) at 8 a. m. 26. Orange-coloured *cirri* at sunset 27. Thunder clouds: a few drops p. m.: much dew. 28. *Cirrocumulus* cloud, very beautiful, interchanging with *cirrostratus*, succeeded by large *cumuli*. In the evening some appearance of a thunder storm far in the N. W. 29. Evening parallel bars of *cirrostratus*, stretching E. and W.: a blush on the twilight. 30. Windy, cloudy.

Aug. 2. Large elevated *cirri*. 3. *Cirrocumulus*, followed by *cirrostratus*: evening overcast: rain by night. 4. Windy, at S.W. by night. *Cumulostrati*, in various quarters, at sunset. 7. Opaque twilight, with *cumulostratus*. 8. Very wet, a. m.; at noon a thunder shower; at 6 p. m. a heavy squall from N.W. with rain and hail; the *nimbus*, as it receded, presenting a perfect and brilliant bow: windy night. 9. Large *cumuli* rose, and at noon inosculated with the clouds in a superior stratum: a thunder shower ensued before 2 p. m., after which appeared the distinct strata again: about 6 p. m. a second thunder shower, long very dense in the S. E., where the bow was conspicuous above an hour. This day was nearly calm. 10. Rain fell again about noon, upon the union of two *strata* of cloud.

RESULTS.

Prevailing winds, westerly.

Barometer:	max. 30·15;	min. 29·35.	Mean 29·835 In.
Thermom.	—— 78°	—— 44°	—— 61°
Evaporation	3·78 In.		
Rain	5·37 In.		

Character of the period changeable, with much rain.

I have the satisfaction to acquaint my readers, that the Meteorological Tables and Remarks, which will hereafter appear in this Journal, will be extracted (as the present has been,) from the journals of Mr. Luke Howard, whose Treatise on Clouds, inserted in the present number, and long known and valued by the public, will make it unnecessary for me to express, in any direct terms, that sentiment of obligation, which myself, and the other cultivators of science, must entertain for his researches. W. N.

X.

Abstract of a Memoir on the Analysis of Vegetable and Animal Substances: by Messrs. GAY-LUSSAC *and* THENARD*.

Mode of analysing vegetable and animal substances.

WHEN we conceived the design of studying the analysis of animal and vegetable substances, the first idea that occurred to us was, to convert, by means of oxigen, vegetable and animal substances into water, carbonic acid, and nitrogen: and on this we fixed our attention. It was evident, that, if we could effect this conversion so as to collect all the gasses, this analysis would attain very great accuracy and simplicity.

Difficulties.

Two obstacles appeared in the way of this: first, the burning of the hidrogen and carbon of these substances completely; and, secondly, the effecting of this combustion in close vessels.

Methods of obviating them.

The first we could hope to surmount only by means of metallic oxides, that easily part with their oxigen, or of the hyperoximuriate of potash. A few trials soon led us to prefer this salt, which succeeded beyond our expectations. It was far from being so easy to surmount the second: for we could not attempt the combustion in a retort filled with mercury; since the retort would have burst, had we burned ever so little in this way. It was necessary therefore to contrive an apparatus, in which we could

Apparatus requisite.

1st, Burn parts of a substance so small, that the vessels should not crack:

2dly, Effect such a number of combustions successively, that the results should be very perceptible: and

3dly, Collect the gasses as they were formed.

Description of an apparatus answering these purposes.

An apparatus of this kind we lay before the class. It is formed of three separate pieces. One is a tube of very thick glass, hermetically sealed at the lower end, and open at the upper; about 2 dec. [7·87 in.] long, and 8 mil. [3·15 lines] in diameter. This has a very small tube, likewise of glass, similar to what would be adapted to a retort to receive gasses, joined laterally to it by means of the blowpipe 5 cent.

* Ann. de Chim. Vol. LXXIV, p. 47. Read to the Institute the 15th of January, 1810.

[1·97 in.]

[1·97 in.] from its aperture. The second is a brass collar, in which the open end of the large glass tube is fixed by means of a cement, that will not fuse under 40° [104° F.] The third piece is a cock of a peculiar construction, in which all the merit of the apparatus consists. The key of this cock is solid, and may be turned into any position, without giving passage to the air; but about the middle of its length it has a superficial cavity, capable of holding a substance the size of a small pea. This cavity is so contrived, that when uppermost it answers to a small vertical funnel, which enters into the nozzle, and forms as it were its extremity; and when lowermost it communicates with the body of the cock, which is perforated, and screws into the brass collar before mentioned. Thus on putting small fragments of any thing into the funnel, and turning the key, the cavity is filled with them, and conveys them, on continuing to turn it, into the body of the cock, whence they fall into the brass collar, and so to the bottom of the glass tube.

Its application. If this matter therefore be a mixture of some vegetable substance with hyperoximuriate of potash in suitable proportion, and if the lower part of the glass tube be sufficiently hot, it will scarcely touch it before it is vividly inflamed; when the vegetable substance will be instantaneously destroyed, and converted into water and carbonic acid, which may be collected over mercury, with the superfluous oxigen gas, by means of the small lateral tube.

Preliminary steps. To perform this operation readily, it is necessary, that the matter should separate entirely from the cavity, and fall to the bottom of the tube. For this purpose it is to be made into small balls, as will presently be described. It is necessary too to inquire, what quantity of hyperoximuriate will be sufficient for burning the vegetable substance completely; and at least half as much more must be used, that the combustion may be perfect.

Preparation of the substance to be analysed.

Analysis of the hyperoximuriate. But of all the preliminary steps the most important is the analysis of the hyperoximuriate employed, for all the calculations of the experiments are founded in great measure on this analysis.

All this being well understood, it will be easy to conceive,

 how

Process described.

how a vegetable substance may be analysed with the hyperoximuriate. Let the substance to be analysed be carefully levigated, and let the hyperoximuriate be levigated separately: weigh the quantity of each, dried at the heat of boiling water, in a very sensible balance; mix them intimately, moisten them, and mould them in cylinders; divide these cylinders into small portions, and round them between the fingers like pills; and lastly expose these to the temperature of boiling water for a sufficient time to render them as dry as the powders were before. If the substance to be analysed be a vegetable acid, it must be combined with lime or barytes, before it is mixed with the hyperoximuriate; the salt thus formed is to be analysed, and account taken of the carbonic acid that remains united with the base after the experiment; in fine, if the substance to be analysed contain any thing foreign to its nature, account must be taken of this also.

Thus we know with precision, that a given weight of the mixture answers to a known weight of hyperoximuriate and the substance to be analysed.

Now, to finish the operation, all that is required is, to bring the bottom of the tube to a cherry red heat; to expel all the air by means of a certain number of balls, which need not be weighed, and which are dropped into it one after another; and then to decompose a quantity accurately weighed, and carefully collect all the gasses in phials filled with mercury, and previously measured.

Proof of its accuracy.

If all the phials be of the same size, they will be filled with gas by equal weights of the mixture; and if the gas be examined, it will be found precisely similar, an evident proof of the extreme accuracy of this mode of analysis.

Caution.

During the whole of the process the tube should be kept at the highest degree of heat it can support without fusion, that the gasses may contain no oxicarburetted hidrogen, or as little as possible. In all cases the analysis should be made over mercury. This is a trial which is indispensable. It is sufficient to mix them with a fourth of their bulk of hidrogen, and to take the electric spark in them. As they include a great excess of oxigen, the hidrogen added, of which account must be taken, burns as well as all the oxicarburetted

Analysis of the gasses.

retted hidrogen they may contain; and thus we acquire a certainty, that they no longer consist of any thing but carbonic acid and oxigen, the separation of which is to be effected by means of potash.

Farther precautions.

But this necessity of raising the temperature so high, obliges us, on the other hand, to take some precautions for preventing the cock from being heated. For this purpose the glass tube is passed through a brick, into which it is luted with clay, which has the advantage, at the same time, of rendering the apparatus firm; and besides, a small hollow cylinder is soldered to the body of the cock, to contain water, or ice, which is still better.

Data.

Thus we have all the necessary data for knowing the proportion of the principles of the vegetable substance. We know how much of it has been burned, for we have its weight to half a milligramme, [about eight thousandths of a grain]; we know how much oxigen was required to convert it into water and carbonic acid, since the quantity is the difference between that contained in the hyperoximuriate and that found in the gasses produced: lastly, we know how much carbonic acid has been formed, and can calculate how much water must have been produced.

Analysis of animal substances.

By following the same method of analysis, we may equally determine the proportions of the constituent principles of all animal substances. But as these substances contain nitrogen; and nitrous acid gas would be formed, if an excess of hyperoximuriate were employed for burning them; only such a quantity must be used, as is sufficient to reduce them completely to carbonic acid gas, oxicarburetted hidrogen, and nitrogen, which are to be analysed in the mercurial eudiometer by the common methods, whence we deduce with precision the proportions of the principles of the animal substance itself.

Small quantities used, but the results accurate.

The mode in which we proceed in the analysis of vegetable and animal substances being exactly known, we may say what is the quantity we decompose, without fear of diminishing the reliance, that may be placed on our results. This quantity extended, at most, to 6 dec. [9·27 grs.] If, however, the least doubt should arise respecting their extreme accuracy, we should remove it by observing, that we filled

filled with gas two and sometimes three phials of the same size in succession, that these gasses were absolutely the same, and always came from the same weight of the substance.

Accuracy of an analysis depends chiefly on the nicety of the apparatus and of the method.

We may add, that the precision of an analysis depends much more on the accuracy of the instruments, and of the methods employed, than in the quantity of the substance on which we operate. The analysis of air is more accurate than any analysis of salts, though it is made on two or three hundred times less matter: because in the former, where we judge of weights by very considerable bulks, the errours to which we are liable are perhaps ten or twelve hundred times less sensible than in the second, where we have not this resource. Now, as we convert into gas the substances we analyse, we bring our analyses not merely to the certainty of ordinary mineral analyses, but to that of mineral analyses of the greatest accuracy; particularly as we collect at least a quart of gas, and in our method of proceeding itself find the proof of an extreme accuracy, and of the most trifling errours.

Vegetable substances already analysed.

By this method, and with all the precautions we have mentioned, we have already analysed sixteen vegetable substances; namely, the oxalic, tartarous, mucous, citric, and acetic acids; yellow resin, copal, wax, and olive oil; sugar, gum, starch, sugar of milk, beech wood, oak, and the crystallizable principle of manna. The results we have obtained seem to us highly interesting, for they have led us to three remarkable laws, to which the composition of vegetables is subjected, and which may be expressed as follows.

Laws of vegetable composition.

1. A vegetable substance is always acid, whenever its oxigen is in greater proportion to its hidrogen than would form water.

2. A vegetable substance is always resinous, or oily, or alcoholic, &c., whenever its oxigen is in smaller proportion to its hidrogen than would form water.

3. Lastly, a vegetable substance is neither acid, nor resinous, but analogous to sugar, gum starch, sugar of milk, woody fibre, or the crystallizable principle of manna, whenever its oxigen is in the same proportion to its hidrogen as would form water.

Thus,

Thus, if we were to suppose, for a moment, that the hidrogen and oxigen were in the state of water in vegetable substances, which we are far from considering as true, vegetable acids would be formed of carbon, water, and oxigen, in different proportions: *Vegetable acids;*

Resins, fixed and volatile oils, alcohol, and ether, would be formed of carbon, water, and hidrogen, also in different proportions: and *resins, &c.*

Lastly, sugar, gum, starch, sugar of milk, woody fibre, and the crystallizable principle of manna, would be formed of carbon and water alone, and would differ only by the greater or less quantity they contained. *sugar, &c.*

This we may show by quoting various analyses of acid and resinous substances, and of substances that are neither acid nor resinous.

A hundred parts of oxalic acid contain. *Constituent principles of oxalic acid,*

Carbon	26·566	or	Carbon	26·566
Oxigen	70·689		Oxigen and hidrogen in the proportions that form water	22·872
Hidrogen	2·745		Oxigen in excess	50·562
	100·			100·

A hundred parts of acetic acid contain *and acetic acid.*

Carbon	50·224	or	Carbon	50·224
Oxigen	44.147		Oxigen and hidrogen in the proportions that form water	46·911
Hidrogen	5·629		Oxigen in excess	2·865
	100			100

Oxalic acid therefore contains more than half its weight of oxigen in excess with respect to its hidrogen; while in acetic acid this excess is not quite three hundredths. *These the two extremes.*

These two acids occupy the extremities of the series of vegetable acids: one is the most oxigenized of them, the other the least. This is the reason why so much nitric acid is required to convert sugar, gum, &c., into oxalic acid; why, on the contrary, many vegetable and animal substances so easily produce acetic acid in a number of instances; and why, in particular, wine is changed into vinegar without the *Explanation of certain facts.*

rmation of any intermediate acid: a phenomenon hi-
rto unexplained, because vinegar was considered as the
most oxigenized of all the acids.

Constituent principles of common resin,

A hundred parts of common resin contain

Carbon	75·944
Hidrogen and oxigen in the proportions that form water	15·156
Hidrogen in excess	8·900
	100

olive oil,

A hundred parts of olive oil contain

Carbon	77·213
Hidrogen and oxigen in the proportions that form water	10·712
Hidrogen in excess	12·075
	100

crystallized sugar,

A hundred parts of crystallized sugar contain

Carbon	40·194	or	Carbon	40·194
Oxigen	52·101		Hidrogen and oxigen in the proportions that form water	59·806
Hidrogen	7·705		Oxigen in excess	0
			Hidrogen in excess	0
	100			100

and beech wood.

A hundred parts of beech wood contain

Carbon	51·192	or	Carbon	51·192
Oxigen	42·951		Hidrogen and oxigen in the proportions that form water	48·808
Hidrogen	5·857		Oxigen in excess	0
			Hidrogen in excess	0
	100			100

Vegetation solidifies water,

These results evince a very important truth, which is, that vegetables, in the act of vegetating, solidify water entire

entire, or its principles: for, all vegetables being almost wholly composed of woody fibres and mucilage, which contain oxigen and hidrogen in the same proportions as water; it is evident, that, being taken into the vegetable, it combines with charcoal to form them. or its principles.

If therefore it were in our power to unite these two substances in all proportions, and to bring their particles to a suitable degree of approximation, we should be able to make with certainty all the vegetable substances, that occupy the mean between acids and resins, as sugar, starch, woody fibre, &c. Requisites to the fabrication of vegetable matters.

Of animal substances we have hitherto analysed only fibrin, albumen, gelatin, and caseous matter. Animal substances analysed.

It follows from our analysis, that, in these four substances, and probably in all similar animal substances, hidrogen is in a larger proportion to oxigen than in water: that, the greater the excess of hidrogen they contain, the greater too is the quantity of nitrogen found in them: that these two quantities are almost in the same proportion as in ammonia; and it is probable, that this proportion, to which we come near, really exists; particularly as we always find a little too much hidrogen, and all the errours, to which we are liable, tend to increase the quantity of this principle. The reader may judge of this from the two following analyses. General conclusions.

A hundred parts of fibrin contain Constituent principles of fibrin.

Carbon	51·675
Hidrogen and oxigen in the proportions that form water	26·607
Hidrogen in excess	5·387
Nitrogen	16·331
	100

A hundred parts of caseous matter contain and of cheesy matter.

Carbon	57·190
Hidrogen and oxigen in the proportions that form water	18·778
Hidrogen in excess	5·680
Nitrogen	18·352
	100

Admitting

mitting this proportion, these substances would corre-
l, with regard to the rank they hold among animal
ances, to the rank occupied by sugar, gum, woody
&c., among vegetable substances: for, as hidrogen
and xigen, the gaseous principles of these, are capable of
ally saturating each other, and forming water; hidro-
n, oxigen, and nitrogen, the gaseous principles of those,
so mutually saturate each other, and form water and
nia: so that carbon, the only fixed principle they all
contain, has no property that acts in this saturation. If we allow ourselves to be guided by analogy, in this point of vi we should compare the animal acids with the vegetable ac he a if there be any that contain nitrogen, vegetable oils and resins; consequently there is not a ent quantity of hidrogen in the uric acid to saturate the en and nitrogen this acid contains, or to form water and ammonia by combining with these two substances; and in animal fats the contrary must occur.

The subject to be pursued. No doubt many more consequences may be drawn from the preceding results: but we reserve for a future paper this inquiry, of the extent and importance of which we are fully aware.

XI.

Chemical Examination of a white, filamentous Substance, found in the Cavities of the Cast Iron that adheres to the Sides of high Furnaces: by Mr. VAUQUELIN*.

Pieces of iron adhering to the sides of the furnace, and containing a white substance,

IN smelting iron ores there are frequently portions of metal, which, beginning to assume the character of iron, and congealing the moment before the iron is drawn off, remain adhering to the sides of the furnace. In these pieces cavities are frequently formed, which are filled with a white filamentous substance, like flexible amianthus.

supposed to be oxide of zinc.

Several metallurgists have spoken of this substance. Grignon in particular hasconsidered it as an oxide of zinc: but

* Ann. de Chim. vol. XXVII, p. 192. Extracted from the Ann. des Museum d'Hist. Nat. An. 7.

but he, no doubt, relied on the external appearance, for it does not contain an atom of this metal.

To satisfy himself whether it were really oxide of zinc, Mr. Vauquelin boiled some with different acids, but none of them had any action on it: they did not dissolve an atom. This led to a doubt of the truth of the assertion of metallurgists respecting it: and the following experiment convinced him, that they were altogether mistaken.

It is not soluble in acids.

Having heated this substance with thrice its weight of caustic potash in a silver crucible, it was completely fused, and the mass produced was entirely dissolved by water.

Treated with potash,

The solution supersaturated with very dilute muriatic acid did not become turbid, but was converted into a white transparent jelly by evaporation, which is never the case with zinc.

and muriatic acid,

When this was perfectly desiccated, and the residuum treated with water, a white powder was obtained, which, when washed and dried, did not differ from the original quantity taken a hundredth and half.

This powder exhibited all the characters of the purest silex. No other earth existed in the liquor from which it had been separated, and not even any sensible quantity of oxide of iron.

it was found to be silex.

The difficulty consisted not in finding the nature of this substance, but how it was formed in the cavities of the iron. How indeed are we to conceive, that the silex, which is always mixed with alumine and lime both in the ores of iron, and in the fluxes employed, should have separated from these earths in a state of such perfect purity, that no perceptible quantity of foreign matter can be discovered with it?

How is it separated?

The filamentous, and as it were crystallized state of this silex announces, that it was converted into vapour by the violence of the fire, and afterward gently condensed in the parts of the furnace that were less hot.

Apparently by sublimation.

This would prove, not only that silex is volatile at a sufficient temperature, but that it is more so than alumine or lime; unless we suppose these two earths to have been raised to a greater height, which is not probable.

XII.

XII.

An Account of the Burrknot Apple. In a Letter to HENRY [illegible]ASTON, *Esq. F. H. S. By the Rev.* JOHN SIMPSON*.

MY DEAR SIR,

Burrknot appletree.

YOUR letter met me on my return home after a month's ramble among the mountains and lakes in Cumberland, and I now send you a short description of the apple tree called here the burrknot. At a proper season I will forward to you a few knots, or knobs of it, for trial, which, put into the ground, will make a long shoot, the following spring; or, if you wish it, I will send you a few knobbed branches with blossom buds upon them, which will bear a little the same year, but you must observe the smaller knobbed branches with blossom buds will not make such fine or handsome trees as the others.

Its good qualities.

The burrknot apple tree† is uncommonly productive. My trees never miss bearing, not being so liable to blight in inclement seasons, as other varieties. The fruit is large, its tints resembling the ribston pippin, and about its size. For culinary uses, it is not inferior to the choicest codlin, and a much better keeper. The tree is not liable to canker, owing, I am persuaded, to its not putting out a tap-root, but spreading its numerous fibres from the knob horizontally, and following the richness of the soil.

It bears in a year's growth.

Our late worthy and valuable friend, Sir Christopher Sykes, observing my trees of one year's growth with fruit upon them, was astonished, and the following year had the pleasure of exhibiting some of the knobbed branches, which I gave him, adorned with fruit in his own garden to his friends, of which you have probably been an eye witness, having visited so frequently in his time at Sledmere. If you wish for any other information that I can give respecting this apple-tree, I shall be happy to send it, and remain,

Dear sir,

Yours very truly,

JOHN SIMPSON.

Rooss, near Patrington,
July 25th, 1808.

* Trans. of the Horticultural Soc. Vol. I, p. 120.

† Specimens of the fruit, and branches of this apple tree from Rooss, which is also plentiful in Lord Hawkesbury's garden at Combe, were exhibited at the meeting of the Society, held Dec. 6th.

XIII.

XIII.

A short Account of a new Apple, called the Spring Grove Codling. By the Right Hon. Sir Joseph Banks, *Bart. K. B. P. R. S. &c.**

AT the request of Mr. T. A. Knight, I beg leave to lay before this Society the opinion formed by my friends and myself last autumn, on the merits of an apple produced by one of his judicious mixtures, which he has done me the honour to call the Spring Grove codling. Spring Grove codling.

In the beginning of September, I received a small box of these apples, which were fully ripe; when baked they had all the quickness and flavour of the best winter apples, and a considerable tinge of red.

All who had tasted the pie agreed, they had not met with any autumn apple which for baking could be compared to this new one. Mr. Knight informs me, that it is ready for use in the month of July, at a season when London geese are probably better than at any other; but when the old English accompanyment of apple sauce was not, till Mr. Knight furnished us with this apple, possible to be obtained; in this view it becomes an addition of importance to the old English kitchen, the cookery of which true Englishmen still prefer to French ragouts, or to Spanish olios. Its season of ripening.

It proves of the burr apple kind, and may be accordingly propagated by cuttings without difficulty, which will bear the next year, as well as by grafting. Mr. Hooker, who colours the Pomona Herefordiensis, has made a very excelcellent representation of this fruit, of which a copy accompanies this communication: as a record in the archives of the Society it may hereafter become a useful, as well as a valuable deposit. The tree grows freely, and bears abundantly. Of the burr apple kind.

* Trans. of the Horticultural Society, vol. I, p. 197.

SCIENTIFIC

SCIENTIFIC NEWS.

DR. Delaroche informs us, that he has made some curi- experiments on radiant heat, which he intended to before the Institute. He ascertained, that the heat itted by radiation is not proportional to the excess of the t iperature of the radiating body above the circumambient medium, but that it increases in a much more rapid ratio. hus, taking the quantity of heat emitted with an excess 37° to be 1°, the quantity emitted with an excess of 900° be at least 70°. He also found, that the quantity of that traverse glass is much greater antity of rays emitted, when the bo hot, than when it is less so; and nature unluminous calorific rays is not identi varies according to the temperature of the source that emits them.

Prize subject. At the request of Mr. Berthollet, the French Institute has proposed for the subject of a prize a determination of the specific heat of gasses.

St. Thomas's and Guy's Hospitals.

Medical and chirurgical lectures. The Winter Courses of Lectures at these adjoining Hospitals will commence the first week of October; viz.

At St. Thomas's.

Anatomy, and the Operations of Surgery, by Mr. Cline, and Mr. Astley Cooper.

Principles and Practice of Surgery, by Mr. Astley Cooper.

At Guy's.

Practice of Medicine, by Dr. Babington and Dr. Curry.

Chemistry, by Dr. Babington, Dr. Marcet, and Mr. Allen.

Experimental Philosophy, by Mr. Allen.

Theory of Medicine, and Materia Medica, by Dr. Curry and Dr. Cholmeley.

Midwifery, and Diseases of Women and Children, by Dr. Haighton.

Physiology, or Laws of the Animal Œconomy, by Dr. Haighton.

Structure

Structure and Diseases of the Teeth, by Mr. Fox.

N. B. These several Lectures are so arranged, that no two of them interfere in the hours of attendance; and the whole is calculated to form a Complete Course of Medical and Chirurgical Instructions.

London Hospital.

Dr. Buxton's Autumnal Course of Lectures on the Practice of Medicine will be commenced on Wednesday, the 2d of October.

Anatomical Theatre, Bristol.

Mr. J. Shute will commence his Winter Course of Lectures on Anatomy, Physiology, and the Principles of and Operations in Surgery, on Tuesday, October 1, at eight o'clock in the morning.

Mr. Vergne has lately analysed the mineral waters of St. Felix de Bagnère, near Condat, in the department of the Lot, and the following were the results. Four pounds ten ounces of the water, evaporated to dryness, left a residuum of 113 grains. From this he obtained Analysis of the mineral water of Bagnère.

Muriat of magnesia	6 grs.
Sulphat of magnesia	41
Sulphat of lime	36
Carbonat of lime	20
Carbonat of iron	1·5
Fatty matter	1

there being a loss of 7·5 grs. The fatty matter had neither taste nor smell; thrown on burning coals, it changed colour, shrunk up like an animal substance, and emitted a very fetid smell of carburetted hidrogen. The heat of the water, taken from the spring at noon on the 21st of June, 1809, was 66·4° F.; and its gaseous products were a moderate quantity of carbonic acid, and still less sulphuretted hidrogen.

The water of the baths of Ussat, near Tarascon, about ten miles from Ax, have been examined by Mr. Figuier, professor of chemistry at Montpellier. He found its heat, taken at several times and at different hours, from 27° R. to 30·5° Water of Ussat.

[92·7° to 100·6° F.]. It contained about a sixth of a cub inch of carbonic acid gas in a pound of water. 12230 nes yielded, by evaporation, 11 grammes of dry resi-, from which were obtained

Muriate of magnesia	0·42 grammes
Sulphate of magnesia	3·38
Carbonate of magnesia	0·12
Carbonate of lime	3·28
Sulphate of lime	3·75
	10·95

The new spring contained rather less both of carbonic acid and of solid residuum, but the difference was trifling.

The mud collected at the bottom of the baths consisted of

Alumine	40 parts
Carbonate of lime	20
Sulphate of lime	10
Oxided or carbonated iron	2
Silex	28
	100

Water of Niederbrunn.

We have also an analysis of the mineral water of Niederbrunn, in the department of the Lower Rhine, by Professors Gerboin and Hecht, of Strasburg. About half a kilogramme, or one pound *, of this water contained

Muriate of soda	1·8 gramme =	27·8 grs.
Sulphat of lime	0·1	1·54
Carbonate of lime, dissolved in Carbonic acid	0·45	6·95
Carbon. of magnesia, the same	0·21	3·24
Carbon. of iron, the same	0·07	1·08
Muriate of magnesia	0·26	4·02
Muriate of lime	0·345	5·33

Augsburg beer.

In Augsburg and its vicinity, which are celebrated for good beer, it is customary to put into each cask a small bag of the root of the geum urbanum, avens, or herb bennet.

* Probably the Strasburg pound = 7277 grs. Eng. C.

A
JOURNAL
OF
NATURAL PHILOSOPHY, CHEMISTRY,
AND
THE ARTS.

OCTOBER, 1811.

ARTICLE I.

On the Destruction of an Enemy's Fleet at Sea by Artillery: by W. MOORE, *Esq., of the Royal Military Academy, Woolwich.*

LEMMA I.

If two Spheres of different Diameters and Different specific Gravities impinge perpendicularly on two uniformly resisting fixed Obstacles and penetrate into them; the Forces which retard the Progress of the Spheres will be as the absolute resisting Forces or Strengths of the Fibres of the Substances directly, and the Diameters and specific Gravities of the Spheres inversely. Law of resistance to a cannon ball.

LET R and r denote the absolute resisting forces of the two substances; F and f the retardative forces; D, d, the diameters of the spheres; Q, q, their quantities of matter; and N and n their respective specific gravities. Then the whole resistances to the spheres, being proportional to the quantities of motion destroyed in a given time, will be as the absolute resisting forces of the two substances and quantities of resisting surfaces jointly; or, as the resisting forces of the substances and squares of the diameters of Proof.

impinging spheres; because the surfaces of sphere as the squares of their diameters; that is $\frac{M}{m} = \frac{R}{r} \times$

.

But in general $\frac{M}{m} = \frac{F}{f} \times \frac{Q}{q}$: Therefore equating these two values of the whole resisting forces we have $\frac{F}{f} \times \frac{Q}{q} = \frac{R}{r} \times \frac{D^2}{d^2}$ and $\frac{F}{f} = \frac{R}{r} \times \frac{D^2}{d^2} \times \frac{q}{Q}$; and since the quantities of matter in spheres are

eir magnitudes and densities,

liameters and densities; it is

$\frac{F}{f} = \ldots \frac{\ldots}{\ldots^3} \times \frac{n}{N} = \frac{R}{r} \times \frac{d}{D} \times$

that is the rding spheres penetrating uni-

re as the absolute strengths of

directly, and the diameter and

res inversely.

Q. E. D.

Lemma II.

Law of the depth to which a ball will penetrate.

The whole Spaces or Depths to which Spheres impinging on differently resisting substances penetrate, are as the Squares of the initial Velocities, the Diameters and specific Gravities of the Spheres directly, and the absolute Strengths of the resisting Substances inversely: or $\frac{S}{s} = \frac{V^2}{v^2} \times \frac{D}{d} \times \frac{N}{n} \times \frac{r}{R}$.

Proof. For by mechanics $\frac{S}{s} = \frac{V^2}{v^2} \times \frac{f}{F}$; and by the preceding Lemma $\frac{f}{F} = \frac{r}{R} \times \frac{D}{d} \times \frac{N}{n}$; therefore by substitution $\frac{S}{s} = \frac{V^2}{v^2} \times \frac{D}{d} \times \frac{N}{n} \times \frac{r}{R}$.

Q. E. D.

These

These being premised, I now proceed to resolve the following important

PROBLEM.

To find a general Formula, which shall express the Charge of Powder for any given Piece of Artillery to produce the greatest Destruction possible to an Enemy's Ship at Sea; it being supposed of Oak Substance of given Thickness, and at a Distance not affecting the initial Velocity of the Shot. To find the charge of powder, that shall do most execution.

By the last of the foregoing lemmata we have generally $V = \left(\frac{S d n R v^2}{s D N r}\right)^{\frac{1}{2}}$. Also the charges of powder vary as the squares of the velocity and weight of the ball jointly. Hence, since it has been determined from experiment, that a charge of half a pound impelled a shot weighing 1lb. with a velocity of 1600 feet per second; we shall, considering V the velocity of any ball impinging on the side of the vessel, have for the expression of the charge impelling it through the space $S = \frac{S R d n v^2 w}{2 D N r s\, 1600^2}$.

Now to apply this in the present instance it is first necessary, that a case be known concerning the penetration of a given shot into oak substance. Such a case is presented at p. 273 of Dr. Hutton's Robins's New Principles of Gunnery. It is there asserted, that an 18 pounder cast iron ball penetrated a block of well seasoned oak (such as ships of war are generally built with) to the depth of 3½ inches when fired with a velocity of 400 feet per second. Making therefore this the standard of comparison for all cases where the object is of oak substance, we shall have for the charge generally,

$$\frac{400^2 \times \cdot 42}{2 \times 1600^2 \times \frac{7}{24}} \times \frac{S R n w}{D N r},$$

or, because the balls are of the same specific gravity, and the substance the same, or $R = r$, and $N = n$; it will be

$$\frac{400^2 \times \cdot 42}{2 \times 1600^2 \times \frac{7}{24}} \times \frac{S w}{D} = \cdot 045 \times \frac{S w}{D}$$

 that

that is, the charge varies as the space to be penetrated and weight of the ball directly, and diameter of the ball inversely.

But the charge by the problem being to produce the greatest effect possible in the destruction of the vessel; S in the above formula must always be put equal to the given thickness of the side; since it is well ascertained, that, for a shot to produce the most damage to any splintering object, such as oak; it must lose all its motion just as it ceases to be resisted by the object, which happens when the ball has forced its first hemisphere out of the farther surface of it. And the quantity of motion destroyed during the penetration of the first hemisphere of the ball into, and the exit of the same out of the object is precisely equal to what would be destroyed during the penetration of the ball through one of its radii, if the quantity of resisting surface was equal to half its entire superficies. Hence the charge in question will be

$$\cdot045 \times \frac{S w}{D}$$

S being the thickness of the side of the ship; w the weight of the ball; and D its diameter.

Example.

Example

An enemy's ship is in sight: required the charge for the 42 pounder guns to destroy her as quickly and completely as possible, when the ships have approached near to each other. The side of the enemy's vessel, a 74, being $1\frac{1}{4}$ foot thick of oak timber.

The diameter of a 42 pounder of cast iron being $= \cdot557$ feet; we get

$$\cdot045 \times \frac{S w}{D} = \cdot049 \times \frac{\frac{7}{4} \times 42}{\cdot557} = 5\cdot93806 \text{ lbs.}$$

or, 5 lbs. 15 ozs. for the weight of the charge sought.

Table

Containing the various charges for the 12, 18, 24, 32, 36, and 42 pounder guns, for producing the greatest effect in all cases of close action: the substance or object being of oak materials from the thickness of 1 foot to that of 5 feet regularly ascending by 1 in the inches.

Tables of charges for different guns for different thicknesses of a ship's side.

Nature of ordnance.	Thickness of the side of the vessel			
	1 ft.	1 ft. 1 in.	1 ft. 2 in.	1 ft. 3 in.
pounder.	lbs.	lbs.	lbs.	lbs.
12	1·439242	1·559178	1·679116	1·799052
18	1·928571	2·089285	2·249999	2·410714
24	2·336650	2·531371	2·726091	2·920813
32	2·830470	3·066343	3·302215	3·538088
36	3·061630	3·316766	3·571901	3·827038
42	3·393180	3·675949	3·958710	4·241475

	16 inches	17 inches	18 inches	19 inches
	lbs.	lbs.	lbs.	lbs.
12	1·918987	2·038926	2·158863	2·278800
18	2·571428	2·732142	2·892856	3·053571
24	3·115533	3·310254	3·504975	3·699696
32	3·773960	4·009833	4·245705	4·481578
36	4·082173	4·337310	4·592445	4·847581
42	4·524240	4·806905	5·089770	5·372535

	20 inches	21 inches	22 inches	23 inches
	lbs.	lbs.	lbs.	lbs.
12	2·398737	2·518674	2·638612	2·758547
18	3·214285	3·374999	3·535714	3·696428
24	3·894417	4·089137	4·283859	4·478580
32	4·717350	4·953323	5·189195	5·425068
36	5·102717	5·357853	5·612988	5·868124
42	5·655300	5·938065	6·220830	6·670262

Nature of ordn.	Thickness of the side of the vessel.			
	24 inches	25 inches	26 inches	27 inches
Pounder 12	lbs. 2·878484	lbs. 2·998420	lbs. 3·118358	lbs. 3·238292
18	3·857142	4·017856	4·178570	4·339284
24	4·673300	4·868021	5.062741	5·257463
32	5·660940	5·896813	6·132685	6·368559
36	6·123260	6·378396	6·633531	6·888668
42	6·786360	7·069125	7·351890	7·634655

	2 ft. 4 in.	2 ft. 5 in.	2 ft. 6 in.	2 ft. 7 in.
12	lbs. 3·358228	lbs. 3·478164	lbs. 3·598100	lbs. 3·718036
18	4·521340	4·682054	4·842768	5·003482
24	5·452184	5·646905	5·841626	6·036347
32	6·504432	6·840305	7·076178	7·312051
36	7·143804	7·398940	7·654076	7·909212
42	7·917420	8·200185	8·482950	8·765715

	2 ft. 8 in.	2 ft. 9 in.	2 ft. 10 in.	2 ft. 11 in.
12	lbs. 3·837972	lbs. 3·957908	lbs. 4·077844	lbs. 4·197780
18	5·164196	5·3[illegible]910	5·485624	5·646338
24	6·231068	6·425789	6·620510	6·815231
32	7·547924	7·783797	8·019670	8·255543
36	8·164348	8·419484	8·674620	8.929756
42	9·048480	9·331245	9·614010	9·896775

	3 ft. 0 in.	3 ft. 1 in.	3 ft. 2 in.	3 ft. 3 in.
12	lbs. 4·317716	lbs. 4·437652	lbs. 4·557588	lbs. 4·677524
18	5·807052	5·967766	6·128480	6·289194
24	7·009952	7·204673	7·399394	7·594115
32	8·491416	8·727289	8·963162	9·199035
36	9·184892	9·440028	9·695164	9·950300
42	10·179540	10·462305	10·745070	11·027835

Nature of ordn.	Thickness of the side of the vessel.			
	3ft. 4in.	3ft. 5in.	3ft. 6in.	3ft. 7in.
pounder	lbs.	lbs.	lbs.	lbs.
12	4·797460	4·917396	5·037332	5·157268
18	6·449908	6·610622	6·771336	6·932050
24	7·788836	7·983557	8·178278	8·372999
32	9·434908	9·670781	9·906654	10·142527
36	10·205436	10.460572	10·715708	10·970844
42	11·310600	11·593365	11·876130	12·158895

	3ft. 8in.	3ft. 9in.	3ft. 10in.	3ft. 11in.
	lbs.	lbs.	lbs.	lbs.
12	5·277204	5·397140	5·517076	5·637012
18	7·092764	7·253478	7·414192	7·574906
24	8·567720	8·762441	8·957162	9·151883
32	10·378400	10·614273	10·850146	11·086019
36	11·225980	11·481116	11·736252	11·991388
42	12·441660	12·724425	13·007190	13·289955

	4ft. 0in.	4ft. 1in.	4ft. 2in.	4ft. 3in.
	lbs.	lbs.	lbs.	lbs.
12	5·756948	5·876884	5·996820	6·116756
18	7·735620	7·896334	8·057048	8·217762
24	9·346604	9·541325	9 736046	9·930767
32	11·321892	11·557765	11·793638	12·029511
36	12·246524	12·501660	12·756796	13·011932
42	13·572720	13·855485	14·138250	14·421015

	4ft. 4in.	4ft. 5in.	4ft. 6in.
	lbs.	lbs.	lbs.
12	6·236692	6·356628	6·476564
18	8·378476	8·359190	8·699904
24	10·125488	10·320209	10·514930
32	12·265384	12·501257	12·737130
36	13·267068	13·522204	13·777340
42	14·703780	14·986545	15·269310

Nature of ordnance.	Thickness of the side of the vessel.		
	4 ft. 7 in.	4 ft. 8 in.	4 ft. 9 in.
Pounder.	lbs.	lbs.	lbs.
12	6·596500	6·716436	6·836372
18	8·860618	9·021332	9·182046
24	10·709651	10·904372	11·099093
32	12·973003	13·208876	13·444749
36	14·032476	14·287612	14·542748
42	15·552070	15·834840	16·117605

	4 ft. 10 in.	4 ft. 11 in.	5 ft. 0 in.
	lbs.	lbs.	lbs.
12	6·956308	7·076244	7·196180
18	9·342760	9·503474	9·664188
24	11·293814	11·488535	11·683256
32	10·680622	13·916495	14·152308
36	14·797884	15·053020	15·308156
42	16·400370	16·683135	16·965900

Explanation of the table. In this table the first column contains the nature of the ordnance, and the numbers in the other columns are their respective charges of gunpowder in pounds, when the thickness of the object to be destroyed is as specified at the top of the columns. If the thickness be given in inches and parts of inches take such parts of the difference between the charge for the given number of inches and the next greater, and add them to the charge first found for the given number of inches for the charge required.

The value of the decimal part of each will be had by multiplying it by 16, the number of ounces in a pound, and pointing off in the product from the right hand towards the left as many places for decimals as are contained in the given decimal, and retaining the number on the left of the point for the ounces, increasing it by $\frac{1}{4}$, $\frac{1}{2}$, $\frac{3}{4}$, or 1, when the first figure of the decimal is 2, 5, 7, or 8, respectively. This hint is merely given for those practitioners into whose hands this table may fall, who are not very conversant in decimal arithmetic.

SCHOLIUM.

Scholium.

This problem is not only of the utmost importance, and practically useful in naval engagements, but in several instances also of military operations; as the bursting open gates of besieged cities with promptitude and effect, and breaking up all fortifications composed of wooden materials, especially those of a splintering nature, to which the foregoing charges apply most correctly. In the case of a naval action, where the object to be penetrated is of oak substance, the ball, by having a small motion when it quits the ship's side, tears and splinters it excessively, breaking away large pieces before it, which are not so easily supplied in the reparation, whereas, on the other hand, if the shot had any considerable velocity when it quitted the side, the effect it produced would be merely a hole, which would be stopped instantly by the mechanic employed for that purpose; and indeed in a great measure by the springiness of the wood itself; for I have seen in his Majesty's dock-yard at Woolwich, captured men of war having a number of shot holes in them almost wholly closed by the wood's own efforts; and that required nothing more than a small wooden peg or a piece of cork to stop them up perfectly. All the mischief therefore the balls can do under such circumstances of extreme celerity is, merely killing those men who may chance to stand in the way of their motion.

The problem applicable to military as well as naval operations.

Advantage of a proper charge in a seafight,

and disadvantage of too much powder.

If any object to be destroyed be so thick, that it cannot be completely pierced by any common engine; or if it be of a very brittle nature, such as stone or brick; then that charge is to be used, which will give the greatest velocity to the shot, to produce the greatest effect. But in many cases of bombardment this charge is by no means to be preferred; for though the effect produced each individual time be greater, yet in any considerable time the whole effect would be less than that from a smaller charge oftener fired, on account of the extreme heat it would give to the engine after a few discharges; and in consequence of which greater time would be required for cooling the gun, and preparing it for farther service.

Cases of thicker substances, or stone or brick walls.

Example

Example II.

Case of bursting open a gate with a 24 pounder.

Required the charge for a 24 pounder shot to burst open the gates of a city with the greatest ease possible, the substance of them being elm 1 foot thick.

Here the object to be penetrated being elm, the small letters in the general formula for the charge

$$\frac{S\,d\,v^2\,w}{2\,D\,s \times 1600^2}$$

must be made to express the several numbers of some experiment made in the penetration of this substance. Now by a mean of many very accurate experiments made by Dr. Hutton at Woolwich, in the years 1783, 1784, and 1785, he found, that a cast iron ball of two inches diameter impinging perpendicularly on the face of a block of elm-wood, with a velocity of 1500 feet per second, penetrated 13 inches deep into its substance; hence we shall have $d = \frac{1}{6}$ ft. $v = 1500$, and $s = \frac{13}{12}$ ft.; also by the question $S = 1$ ft. $D = \cdot 46$, and $w = 24$lbs. Therefore

$$\frac{S\,d\,v^2\,w}{2\,D\,s \times 1600^2} = \frac{1 \times \frac{1}{6} \times 1500^2 \times 24}{2 \times \cdot 46 \times \frac{13}{12} \times 1600^2} = \frac{45 \times 9}{104 \times 1 \cdot 11}$$

$= 3\cdot 50831$ lbs. or 3 lbs. $8\frac{1}{8}$ ozs. for the weight of the charge required in this case.

Retaining the experiment of Dr. Hutton as a standard for all cases where the object to be penetrated is of elm, we shall get by reduction

$$\frac{S\,d\,v^2\,w}{2\,D\,s \times 1600^2} = \cdot 0676 \times \frac{S\,w}{D},$$

the charge for any piece of artillery the diameter of the shot of which is D, and weight w; S being the thickness of the object as before.

A gate may be burst by the recoil of a gun.

It is not unworthy of remark, that the gate of a besieged place, or any like things, might be effectually broken open by the gun itself charged only with powder, by placing it close to the gates with its muzzle from them; the momentum of recoil being generally sufficient to force such objects completely.

Of great importance in close fighting.

From the circumstance, that no English admiral or commander ever commences firing till his ships are about to be grappled

grappled with those of the enemy, or until they have approached them so nearly as to affect in no sensible degree the first force of the shot; the above paper has, it is presumed, as much claim to utility as any that has ever yet been offered to the navy in the science of gunnery; and even if the vessels be not so closely in action, but are fighting at the distance of about 30 or 40 feet from each other, no danger would result from the above charges, provided that the shot impinged perpendicularly on the side of the vessel; on account of the splitting of the timber in some degree, which would make ample compensation for the defect of velocity occasioned by the resistance of the medium.

It is impossible to deduce charges, that shall produce with certainty the effect above stated, when fired at any considerable distance from the ship. The uncertainty of the impact being perpendicular from the unsteadiness of the vessels renders the thing at once nugatory, without any consideration of the *real* resistance of the medium to the ball, and the deflection of the latter from a right lined direction. If the obliquity of the impact be given, or can be determined, then, the problem being otherwise rightly solved, a charge can be found, which shall answer the same purpose as those above given; but, if this be impossible (which it most decidedly is), then will the problem be at best but speculative upon certain hypotheses. Distant firing.

I shall however give an investigation of the problem on the principles of resistance generally allowed, and then conclude the subject by a few observations.

Problem II.

To determine the same as in the last Problem, when the Engine is at any considerable Distance from the Object, and the Resistance of the Air taken into the account. To find the charge, that shall do most execution at a distance.

Here, as in the former proposition, the Velocity $V = \left(\frac{S\,dr^2}{Ds}\right)^{\frac{1}{2}}$ is to be esteemed the velocity of impact. Now on the principle of resistance just adverted to, which considers the fluid as infinitely compressed, and the particles thereof perfectly

perfectly nonelastic and affording no resistance to the body but what arises from their inertia. If a denotes the first or initial velocity; x the distance of the gun from the object; $e = 2.71828$ the number the hyperbolic log. of which is unity; and $b = \frac{3n}{8ND}$ where N and n represent the respective specific gravities of the ball and medium, we shall have

$$a = V e^{bx}$$

(See Dr. Hutton's elegant Exercises on Forces, Prob. 31, and most works on fluxions and mechanics). Hence by the law of variation of the charges, and proper substitution, the true expression for the charge in question will be

$$\frac{S d v^2 w e^{\frac{3nx}{4ND}}}{2 D s\, 1600^2}$$

for a perpendicular impact, and

$$\frac{S d v^2 w e^{\frac{3nx}{4ND}}}{2 D s f\, 1600^2}$$

for an oblique one; f being the sine of the angle of incidence; the space to be described in this case being the hypothenuse of a right angled triangle; when the effect is the same.

Example.

Charge for a 42 pounder at 100 yards distance.

Resuming the first of the foregoing examples; what must be the charge of gunpowder to cause the shot to produce the same effect in the vessel when fired at the distance of 300 feet from it?

Substituting for the several letters in the general expression for the charge

$$\frac{S d v^2 w e^{\frac{3nx}{4ND}}}{2 D s\, 1600^2}$$

their proper numerical values, namely

S =

$$\left.\begin{array}{l} S = 1\tfrac{1}{4}\text{ ft.} \\ s = \tfrac{13}{12}\text{ ft.} \\ d = \tfrac{1}{6}\text{ ft.} \\ D = {\cdot}557\text{ ft.} \\ v = 1500\text{ ft.} \\ x = 300\text{ ft.} \\ w = 42\text{ lbs.} \\ N = 7\tfrac{1}{3} \\ n = {\cdot}0012 \end{array}\right\}$$

we get $\dfrac{S d v^2 w c^{\frac{snx}{4ND}}}{2 D s\, 1600^2} =$ 9·530695 lbs. or 9 lbs. 8½ ozs. nearly for the weight of charge sought; being 3 lbs. 9½ ozs. more in this case than when the vessels are in close action.

Hence not only is the destruction of the vessel more certain when the firing commences just as the ships touch each other but a great saving of powder takes place beside, insomuch, that not more than two thirds of the quantity is expended, that would be required at the distance of 300 feet.

From this circumstance then, and the impossibility of solving the problem rightly from the various causes already enumerated, the effects of which are not reducible to any regular laws; we conclude, that the foregoing table of charges for close fighting is the only one, that can be of the smallest service in practice, and that all attempts at others must be rendered completely futile from the nature and constitution of things.

II.

Correction of an Errour in a former Paper on the Motion of Rockets. By W. Moore, *Esq. In a Letter from the Author.*

To Mr. NICHOLSON.

SIR,

I TAKE the earliest opportunity of correcting the errour so obligingly mentioned by *Zeno* in the last number of your Journal: into which I inadvertently fell in my paper concerning Rockets for July last. On the motion of rockets.

Conceive Q R and K n (Pl. 8, fig. 2,) erased from the diagram, and Q W drawn perp. to T n produced in the plane T Q W; also, draw W R perp. to T P and join P W which will be perp. to T W. Then calling T P unity, T Q will be $= f$ (the same substitution for the several angles remaining as before); also sine ∠ T Q W being expressed by $\frac{r}{r}$ by Trig. $TW = \frac{fx}{r}$: hence $PW = (TP^2 - TW^2)^{\frac{1}{2}}$

On the motion of rockets.

$= \left(1 - \frac{f^2 x^2}{r^2}\right)^{\frac{1}{2}} = \frac{(r^2 - f^2 x^2)^{\frac{1}{2}}}{r}$ which in the present case is equal to the sine of the angle P T W or P W R.

Now because of the oblique action of the fluid against the cylinder, (considering the fluid in motion, and the solid at rest) its force on this account will be diminished in the ratio of radius to the cube of the sine of the angle of incidence, or as 1 to f^3. Therefore considering T P the representative of the force of a particle so diminished $\left(= \frac{n v^2 f^3}{4g}\right)$; its efficacy to move the cylinder in the direction P T will be P R and $= \frac{n v^2 f^3}{4g}$. sin. ∠ P W R $=$ $\frac{n v^2 f^3}{4g} \cdot \frac{r^2 - f^2 x^2}{r^2} = \frac{n v^2 f^3}{4 g r^2} \cdot (r^2 - f^2 x^2)$. Therefore the fluxion of the force of the fluid on F T will be

$$\frac{n v^2 f^3}{4 g r^2} \cdot (r^2 - f^2 x^2) \cdot \frac{r \dot{x}}{(r^2 - x^2)^{\frac{1}{2}}} \cdot \frac{(r^2 - f^2 x^2)^{\frac{1}{2}}}{r}$$

or

$$\frac{n v^2 f^3}{4 g r^2} \cdot \frac{\dot{x}\,(r^2 - f^2 x^2)^{\frac{3}{2}}}{(r^2 - x^2)^{\frac{1}{2}}}$$

the fluent of which is

$$\frac{n v^2 f^3}{4 g r^2} \cdot \left(r^2 x - \frac{3 f^2 - 1}{6} x^3 + \frac{3 (f^2 - 1)^2}{40 r^2} x^5 + \frac{(f^2 + 5) \cdot (f^2 - 1)^2}{112 r^4} x^7 + \&c.\right)$$ which when $x = r$ is $\frac{n v^2 r f^3}{4 g}\left(1 - \frac{3 f^2 - 1}{6} + \frac{3 (f^2 - 1)^2}{40} + \frac{(f^2 + 5) \cdot (f^2 - 1)^2}{112} + \&c.\right)$. This therefore is the effective force of the fluid on the quadrantal arch F T S. Hence the force on the whole semicylindric surface m D v r B s

$$= \frac{n v^2 r h f^3}{2 g}\left(1 - \frac{3 f^2 - 1}{6} + \frac{3 (f^2 - 1)^2}{40} + \frac{(f^2 + 5) \cdot (f^2 - 1)^2}{112} + \&c.\right)$$ which is also the resistance to the cylinder when this moves in the fluid at rest, as far as relates to that surface. Q. E. I.

Yours, &c.

W. MOORE.

III.

On a Property of reflected Light: by Mr. Malus*.

Reflected or refracted rays generally similar to direct,

When a solar ray is reflected, or refracted, it retains in general its physical properties; and if it be subjected to new trials, it comports itself in the same manner, as if it issued directly from the luminous body. The prism, while it disperses the coloured rays, only changes their respective directions, without altering their nature. There are circumstances however, in which the influence of certain bodies impresses on the rays they reflect, or refract, characters and properties which they carry with them, and by which they are essentially distinguished from direct light.

but not always.

Double refraction.

The property of light I am about to describe is a modification of this kind. It had already been perceived in a particular circumstance of the doubling of images exhibited by calcareous spar: but, the phenomenon resulting from it having been ascribed to the properties of this crystal, no one suspected, that it might be produced, not only by all bodies that afford a double refraction, but by all other diaphanous substances, whether solid or liquid, and even by opake bodies.

A ray of light received on Iceland crystal is divided into two.

Plane of the principal section passes through them.

The rays refracted ordinarily and extraordinarily.

If a ray of light be received perpendicularly on the face of a rhomboid of calcareous spar, this ray is divided into two pencils, one continued in the direction of the incidental rays, the other making with it an angle of a few degrees. The plane that passes through these two rays has several peculiar properties, and is called the plane of the principal section. It is always parallel to the axis of the integrant particles of the crystal, and perpendicular to the natural and artificial refractive surface. When the incident ray is inclined to the refractive surface, it is equally divided into two pencils; one refracted according to the ordinary law, and the other according to an extraordinary law, which depend on the angles that the incident ray forms with the refractive surface and the principal section. When the face of emergence is parallel to that of incidence, the two emergent rays are

* Mém. de la Soc. d'Arcueil, vol. II, p. 143.

parallel

parallel to the incident ray, because each ray undergoes the same kind of refraction at the two opposite faces.

The two rays received on another crystal are not divided, when their principal sections are parallel.

If now we receive on a second rhomboid, the principal section of which is parallel to that of the first, the two rays that have already passed through this, they will no longer be divided into two pencils, as rays of direct light would. The pencil from the ordinary refraction of the first crystal will be refracted by the second according to the law of the ordinary refraction, as if this crystal had lost the faculty of doubling images. In like manner the pencil from the extraordinary refraction of the first crystal will be refracted by the second according to the law of the extraordinary refraction.

But by altering the position of one of the crystals they are divided,

and again reduced to two.

If, the first crystal remaining fixed, we turn the second so, that the face of incidence shall remain parallel with itself, each of the two rays arising from the refraction of the first crystal begins to divide itself into two pencils; so that one portion of the ray from the ordinary refraction, for example, begins to be refracted extraordinarily, which produces four images. Finally, after a quarter of a revolution, the pencil from the ordinary refraction of the first crystal is entirely refracted extraordinarily by the second; and, vice versa, the pencil, from the extraordinary refraction of the first crystal is wholly refracted according to the ordinary law by the second; which again reduces the number of images to two. This phenomenon is independent of the angles of incidence, since during the movement of the second crystal the refractive faces of the two rhomboids preserve the same inclination toward each other.

Distinction between direct and refracted light.

Thus the character that distinguishes direct light from light that has been subjected to the action of a crystal is, that the one constantly possesses the faculty of being divided into two pencils, while in the other this faculty depends on the angle comprised between the plane of incidence and that of the principal section.

Light affected in the same way by all double refracting substances.

This faculty of altering the character of light, and of impressing on it a new property, which it carries with it, is not peculiar to the Iceland spar: I have found it in all known substances that double images; and, what is remarkable in this phenomenon, it is not necessary for its production,

production to employ two crystals of the same kind. Thus the second crystal, for example, may be carbonate of lead, or sulphate of barytes; the first may be a crystal of a sulphur, and the second of rock crystal. All these substances comport themselves with one another in the same manner as two rhomboids of calcareous spar. In general this propensity of light to be refracted in two pencils, or in one only, depends solely on the respective positions of the axis of the integrant particles of the crystals employed, be their chemical principles what they may, and of the natural or artificial faces, on which the refraction is produced. This result proves, that the modification light receives from these different substances is perfectly identical.

Method of rendering the phenomena more evident.

To render the phenomena I have described more sensible, the flame of a taper may be viewed through two prisms of different substances, possessing the property of double refraction, placed on each other. In general we shall perceive four images of the flame: but, if we turn one of the prisms slowly round the visual ray as an axis, the four images will be reduced to two, as often as the principal sections of the contiguous faces become parallel, or cut each other at right angles. The two images that disappear do not lose themselves in the other two; we perceive them gradually become extinct, while the other acquire increased intensity. When the two principal sections are parallel, one of the images is formed by rays refracted in the ordinary way by the two prisms, and the other by rays refracted extraordinarily. When the two principal sections are perpendicular, one of the images is formed by rays refracted ordinarily by the first crystal, and extraordinarily by the second; and the other by rays refracted extraordinarily by the first crystal, and ordinarily by the second.

Light affected in a similar way by all transparent bodies,

and even by opake ones.

Not only all crystals, that double images, are capable of giving light this faculty of being refracted in two pencils, or in one only, according to the position of the refractive crystal; but all transparent bodies, whether solid or liquid, and even opake bodies themselves, can impress on the luminous particles this singular disposition, which seemed to be one of the effects of double refraction.

hen a pencil of light traverses a transparent substance, rtion of the rays is reflected by the refractive surface, nother portion by the surface of emergence. The cause is partial reflection, which has hitherto escaped the rches of natural philosophers, seems, in several circumes, to have some analogy with the forces that produce ouble refraction.

From water.

r example, light reflected by the surface of water u... r an angle of 52° 45′ with the perpendicular has all the cters of one of the pencils produced by the double ction of a crystal of calcareous spar, the principal ... or perpendicular to the plane, ... nt ray and the reflected ray, ... of reflection.

... ceived on any crystal, that has ... ages, and the principal section ... arallel to the plane of reflection, it will not ... to two pencils, as a ray of direct light would ... been; but it will be refracted entire according to the ... ry law, as if the crystal had lost the faculty of ...ng images. If, on the contrary, the principal section of the crystal be perpendicular to the plane of reflection, the reflected ray will be refracted entire according to the extraordinary law. In the intermediate positions it will be divided into two pencils according to the same law, and in the same proportion, as if it had acquired its new character by the influence of the double refraction. The ray reflected by the surface of the liquid therefore, under this circumstance, has all the characters of an ordinary ray formed by a crystal, the principal section of which is perpendicular to the plane of reflection.

This phenomenon analysed.

To analyse this phenomenon completely, I placed a crystal so that its principal section was vertical; and after having divided a luminous ray by means of the double refraction, I received the two pencils proceeding from it on the surface of water, at an angle of 52° 45′. The ordinary ray, in being refracted, gave up to the partial reflection a portion of its particles, as a pencil of direct light would have done; but the extraordinary ray penetrated the liquid entire, and none of its particles escaped refraction. On the contrary

contrary, when the principal section of the crystal was perpendicular to the plane of incidence, the extraordinary ray produced alone a partial reflection, and the ordinary ray was refracted entire.

Different bodies produce the effect at different angles.

The angle under which light experiences this modification in being reflected at the surface of different transparent bodies is not the same in all. In general it is greatest in those that refract light most. Above and below this angle a part of the ray is more or less modified, and in a manner analogous to what takes place between two crystals, the principal sections of which cease to be parallel or perpendicular.

Simple exhibition of the phenomenon.

If we would simply observe this phenomenon, without measuring it accurately, we have only to place before a taper the transparent body, or the vessel containing the liquid to be subjected to experiment. We must then observe through a prism of flint glass the image of the flame reflected at the surface of the solid or the liquid, and in general two images will be seen: but on turning the crystal round the visual ray as an axis, one of the images will be seen to grow faint in proportion as the other increases in intensity. Beyond a certain limit, the image that had grown weak begins to renew its intensity at the expense of the second. At the point where the intensity of the light is nearly a minimum, we must move the reflecting body nearer to the taper, or farther from it, till the angle of incidence is such, that one of the two images wholly disappears. This distance being found, if we continue to turn the prism slowly, we shall perceive, that one of the two images becomes extinct alternately at every quarter of a revolution.

The phenomenon takes place in rays reflected in the interior of a substance.

The phenomenon I have mentioned in the rays that are reflected under a certain angle at the surface of a transparent body takes place likewise, but under a different angle, with the pencils reflected interiorly by the surface of emergence; and the sine of the first angle is to the sine of the second as the sine of incidence to the sine of refraction. Thus, if we suppose the face of incidence and the face of emergence parallel to each other: and the angle of incidence such, that the ray reflected at the first surface presents the phenomenon I have described; the ray reflected at the

id surface will be modified in the same manner. If the ent ray be such, that all its particles escape the partial reflection and pass through the face of entrance, they will equally escape by traversing the face of exit. This new property of light affords the means of measuring with precision the quantity of rays absorbed at the surface of diaphanous bodies, a problem, which the partial reflection rendered almost impossible to be solved.

Light reflected from the surface of a doubly refracting body.

When a body, that produces a double refraction, reflects the light at its first surface, it comports itself like a common transparent substance. The light reflected under a certain angle of incidence acquires the property I have described; and this angle is independant of the position of the principal section, which influences only the double refraction, or the reflections that take place in the interior of the crystal.

Rays reflected interiorly exhibit peculiar phenomena.

In fact, the rays that are reflected interiorly at the second surface exhibit peculiar phenomena, which depend both on the refractive power, and the properties of reflected light that I have already described.

When a pencil of light has been divided into two rays at the first surface of a rhomboid of calcareous spar, these two rays issue out by the second face in two pencils parallel to the incident ray, because each of them experiences at that face the same kind of refraction as at the first face. It is not the same with reflected light. Though the ray refracted ordinarily at the first surface is refracted ordinarily at the second, it is nevertheless reflected at this surface in two pencils, one ordinary, the other extraordinary. In like manner the ray refracted extraordinarily is reflected in two others; so that there are four reflected rays, while there are but two emergent. These four rays, in returning to the first face of the crystal, issue out in four parallel pencils, which make with this face the same angle as the incident ray, but in a contrary direction, and are parallel to the plane of incidence. To connect this kind of reflection with that of double refraction, we must conceive at the two points of emergence of the second face two incident rays, making with this face the same angle as the emergent rays, but in the opposite direction. These two rays, by their refraction

fraction through the crystal, will produce four pencils, which will follow precisely the course of the reflected rays. Thus the law of the double refraction being known, that of the double reflection may easily be deduced from it.

I shall now proceed to that kind of phenomenon, which is the subject of this paper; and which relates, not to the law according to which the rays are directed, but to the quantity and properties of the light they contain. Quantity and properties of the light.

Let us suppose the angle of incidence to be constant, and the crystal placed horizontally. If we turn the rhomboid round the perpendicular, so as to approximate its principal section to the incident rays, we shall perceive a gradual diminution of intensity in the ordinary ray reflected extraordinarily, and of the extraordinary ray reflected ordinarily. In fine, when the plane of the principal section coincides with the incident ray, these two reflected rays disappear entirely, and nothing remains but the ordinary ray reflected ordinarily, and the extraordinary ray reflected extraordinarily. The latter however has much less intensity than the former.

If now, the incident ray continuing to be included in the principal section, we increase or diminish the angle of incidence, till it becomes 56° 30′, the latter reflected ray will disappear altogether; and only that, which has been refracted ordinarily, and reflected ordinarily, will remain. Beyond or within this angle, the extraordinary ray reflected extraordinarily will reappear with an intensity proportional to the remoteness from this angle. The angle of incidence I have mentioned is that, under which a ray reflected at the first surface would have acquired the property of being divided into two pencils, or remaining in one, as takes place at the surface of any other transparent body. The preceding phenomenon may easily be connected with the experiment, in which water was taken for an example: for if we let fall on the surface of the rhomboid, under an angle of 56° 30′, or thereabout, a ray disposed to be refracted only in one extraordinary pencil, this ray will produce no partial reflection at the first surface; which seems to explain, why it produces none at the second.

However, it is not the same, when the plane of incidence makes

s a sensible angle with the principal section. If the ist mentioned be made to fall in this plane, under an angle of 56° 30', or near it, it will comport itself at the first surface as in the preceding case, it will traverse it without any reflection: but at the second surface it will be reflected in two pencils, which will attain their maximum of intensity, when the plane of incidence is perpendicular to the principal section.

It is obvious, that the light reflected at the second face does not comport itself here as in the preceding case, because in the first experiment the incident ray refracted and reflected is still in the same plane, while in the last the repulsive force, that produces the extraordinary refraction, turns the light away from the plane of incidence, so that it ceases to be similarly circumstanced with respect to the forces that act on it.

Light from the partial reflection of opake bodies.

If we examine the light that proceeds from the partial reflection of opake bodies, as black marble, ebony, &c., we shall equally find an angle, at which this light enjoys the properties of that which has traversed a crystal of Iceland spar. Polished metals appear to be the only reflecting substances, that do not seem capable of producing this phenomenon: but, if they do not impress this peculiar disposition on luminous rays, they do not alter it, when they have already acquired it by the influence of another substance.

This property is preserved also by pencils, that traverse substances which refract light singly.

Reflection from metallic mirrors.

In the second part of this paper* I shall describe the circumstances, under which, by means of reflection from metallic mirrors, the mutual disposition of the particles of a ray, either ordinary or extraordinary, may be so changed, that some shall always be refracted ordinarily, while the others are refracted extraordinarily. The examination of these different circumstances will lead us to the law of these phenomena, which depends on a general property of the repulsive forces that act on light.

* This will appear in our next. C.

IV.

IV.

Experiments on the Transmission of Sound through solid Bodies, and through Air in very long Tubes: by Mr. Biot*.

It has long been known, that air is not the only medium, in which the phenomenon of sound may be produced and transmitted. All bodies enjoy this property, when they enter into a vibratory motion: and as, even in the most solid substances, the elasticity of the ultimate particles appears to be extremely great, it follows, that sound may be produced and transmitted in all bodies, when they are suitably agitated. This result is confirmed by a great number of daily observations. The miner, when excavating his gallery, hears the strokes of the miner opposed to him: and thus judges of his direction. Stone, wood, metals, and even water, transmit sound: and Franklin assures us, that he has heard under water, at the distance of half a mile, the sound of two stones struck against each other. Several too have observed, that the velocity of sound is much greater in solid bodies, than in the air. Experiments of this kind were made in Denmark on a wire extended horizontally 600 feet. A piece of sonorous metal, suspended from one extremity of this wire, was struck gently; and a person at the other extremity holding it between his teeth, or applying it to some solid part of the organ of hearing, heard two distinct and successive sounds. The first and most rapid was transmitted by the wire: the second through the air: and from their interval, compared with the known velocity of sound in air, it was found, that the sound transmitted by the metal arrived almost instantaneously. These experiments were repeated in England by the Royal Society, and similar results were obtained, but I do not know the precise quantities found. Mr. Hassenfratz too made experiments on the same subject in the quarries at Paris, with Mr. Gay-Lussac.

Sound produced and transmitted by other bodies beside air:

as the ground,

water,

a wire of 600 feet.

Experiments in stone quarries.

* Mém de la Soc. d'Arcueil, vol. II, p. 405. Read to the Institute November, 1808.

c. A stroke of a hammer against the side of the gallery produced two sounds, which separated at a certain distance, and that transmitted by the stone arrived first. This separation too was observed, when the sound was transmitted through iron bars, or wooden rails of different lengths, and no perceptible interval could be distinguished between giving the stroke and hearing the sound.

None of these show the precise velocity in solids.

All these experiments are well adapted to show the great velocity, with which sound is conveyed through solid bodies, but they were made on lengths not sufficient to afford a measure of this velocity, or even to give a precise idea of it.

Attempt to ascertain it by their vibrations.

An ingenious philosopher, whom we have now the pleasure of having at Paris, Mr. Chladni, author of some very fine experiments on the vibrations of solids, has proposed a method of estimating the transmission of sound through their substance. It consists in causing a rod of any substance, of a given length, to vibrate by friction: when the tone produced by the rod, compared with that of a column of air of the same length, will give the ratio of the velocities of the transmission of sound through air, and through the substance of which the rod is formed. In fact, we readily perceive from the theory, that the velocity of the longitudinal oscillations of a body and that of the sound transmitted through it are proportional to one another: but it is necessary to be certain, that the whole rod vibrates so as to give its fundamental note, without dividing itself into its aliquot parts: for such a separation, heightening proportionally the tone, would give a velocity of sound proportionally above the truth. In this way Mr. Chladni found, that the velocity of sound in certain solid bodies is 16 or 17 times as great as in air. The most elastic substances are iron, and fir with very straight fibres, when it is rubbed longitudinally.

16 or 17 times as great as in air.

Experiments made in the aqueducts forming at Paris,

The construction of the aqueducts and conduits, which is at present carrying on for the embellishment of the capital, has furnished me with means of making experiments of this kind on a much greater length, than any of those who have gone before me have had at their disposal. It was besides a subject of curiosity, to learn the effects and reach of the human voice in very long cylindrical tubes. Such

were

were the objects of the following experiments. Some of them were made by Mr. Bouvard and me, others by one of us alone. Mr. Malus, colonel of engineers, was likewise present at many of them. In all of them we were assisted by Mr. Martin, maker of nautical watches, a very ingenious and attentive artist, who was particularly appointed to give instantaneously, at determinate seconds, the stroke that was to produce the sound.

The sonorous body, on which we operated, was formed by a series of cylindrical tubes of cast iron, of as equal dimensions as possible, and the mean length of which I found to be 2·515 met.* [8 feet 3 in. nearly]. This I found by measuring the whole length of twelve cylinders placed end to end. The tubes are separated by leaden rings covered with tarred fustian: but they are pressed together by strong screws, so that the rings are forcibly compressed, and so close a contact produced, that no water can escape. The mean thickness of each ring is 0·014286 met. [0·562 of an inch], as I found by measuring twelve. The whole series of cylinders forms a curved line, which has two inflexions about the middle of its length: but they were not all joined together at once, and we made our experiments on different successive lengths, as will be seen in my report of them.

Which consist of a series of iron pipes.

The first were made by Mr. Bouvard and myself on 78 cylinders, forming a length of 196·17 met., to which must be added 1·1 for the 77 rings, giving a total length of 197·27 met. [215·587 yds]. The following were the phenomena we observed.

1st set of experiments, on a length of 215 yards.

In the last cylinder was placed a ring of iron, of the same diameter as the cylinder, and having in its centre a bell without a clapper, and a hammer that could be let fall at will. The hammer, as it struck the bell, struck also the cylinder, with which it formed a communication by means of the iron ring. Two sounds must therefore be heard, one transmitted by the cylinder, the other by the air.

Apparatus.

In fact they were heard very distinctly by applying the ear to the cylinder, and even without this. They appeared

Mode of experimenting.

* All the measures employed in this paper are expressed in metres; and the time in seconds of the sexagesimal division.

sensibly

ly in unison. The first and more rapid was trans-
d by the substance of the cylinder, the second by the
Strokes of a hammer on the last cylinder likewise pro-
ed this transmission. We observed attentively with
lf econd chronometers the intervals between the two
s transmitted. We even employed successively sexa-
imal and decimal watches, to vary the numbers observed.
Thus we found

Differences in velocity of the solid and the air.

In 11 observations		0·527″ .	Bell.
22		0·555″ .	Hammer
20		0·544″ .	Bell.
53 observations.	Mean	0·542″	

The interval given by the hammer, and by the bell, appeared to us absolutely the same, without any sensible difference. For this reason we have united them in the same mean. Their tones however were very different. Thus in solid bodies, as in the air, the tone makes no difference in velocity of the sound.

Velocity of transmission through the solid calculated.

The temperature of the air during the experiment was 11° [51·8° F.]. The barometer was about 0·76 [29·9 in.]. In similar circumstances the velocity of sound in the air is 340·84 met. [372·487 yds] according to the experiments of the academy, which give 334·02 met. [365·034 yds] for the velocity under the same pressure, and at the temperature of melting ice. For the distance of 197·27 [215·587 yds] therefore, that at which the experiment was made, the time of transmission of the sound by the air was ...... 0·579″
The interval observed between the two sounds was.. 0·542″

Difference, or time of its transmis. thro' the metal.. 0·037″

We do not pretend to give this small difference as exact, since the slightest errour would have a considerable influence on it, but it proves, that the transmission was not absolutely instantaneous.

2d set of experiments, on a

The second set of experiments was made by Messrs. Bouvard and Malus on twice the former number of cylinders, or a length

length of 394·55 met. [431·184 yds]. At this distance the time of transmission through air would be 1·158″ by calculation, supposing the temperature still 11° [51·8° F.]. The interval between the two sounds, deduced from 64 experiments, was found to be 0·81″. The difference therefore, or 0·348″, was the time of transmission through the solid. This appears much too great, if we compare it with the preceding experiments, and on those that follow, which were made on nearly triple the length. The latter would not permit us to suppose a longer time than 0·125″ for the transmission through the solid, which would give an errour of 0·223″ in the observation. But, beside that it is extremely difficult to answer for such quantities, when the instant of observation does not coincide exactly with a beat of the watch, it must be remarked, that the whole length of the pipe might be far from being at the same temperature, which might occasion currents of air, that would influence the velocity of the sound. For instance, in the present case, if we were to admit the transmission of sound through air as it results from the observations of the chronometer made by Messrs. Martin and Bouvard at the points of departure and arrival, it would be found equal only to 1·07″, or 0·088″ less than the truth, which gives 0·26″ for the time of the transmission of the sound through the solid; and the excess of this result over those that follow, being no more than 0·135″, is more easily reconcilable with errours of observation.

Length of 431 yards.

Interval.

Time of transmission through the solid.

This apparently much too great.

Finally, the experiments now to be related were made by Mr. Martin and myself, on a series of 376 cylinders, which, with their joints, formed a length of 951·25 m. [1039·575 yds] of which the joints alone occupy 5·61 m. [6·131 yds]. I satisfied myself at different times, and by more than 200 experiments, either with the hammer or the bell, that the interval between the two sounds transmitted by the metal and by the air, was exactly 2·5″; and I found no sensible variation in this quantity. I made Mr. Martin observe the interval also, without letting him know my results, and he found the same. Now, at the distance of 951·25 met. [1039·575 yds], the temperature being 11° [51·8° F.], the time

3d set of experiments, on a length of 1040 yards nearly.

Interval.

[time of] transmission of the sound through the air would be [by] calculation 2·79″: and if we substract from this 2·5″, the interval observed between the two sounds, there will remain 0·29″ for the time of transmission through the metal to this distance. From the care with which I repeated these observations, and from the exact coincidence of the five beats of the half-second chronometer with the interval between the two sounds, I believe, that this result may be considered as a very near approximation.

Velocity calculated.

Still however it may be objected, that the velocity of the sound in air deduced from calculation might differ a little from what really took place in the pipe, owing to variation of temperature. This would leave some uncertainty with respect to the result, and particularly as to the precise quantity. I sought therefore to verify it directly in another way, and accomplished it as I shall relate.

As this indirect method might be questioned,

I stationed Mr. Martin at one extremity of the pipe with a half-second watch, while I remained at the other with a similar watch, which was carefully compared with the former at the beginning and end of the experiments: though this comparison could have no influence on the results, as will soon appear. When Mr. Martin's watch was at 0″ or 30″, he struck with a hammer on the last cylinder, near which he was stationed: and when my watch was at 15″ or 45″, I answered him by a similar stroke. We each watched the arrival of the sound transmitted to us, and noted down the time. We were very attentive to strike precisely at the appointed second; and this, with a little practice, we could readily do, as the series of our observations will show. Now, whatever the difference of the watches might be; and even if it were variable, provided there was no sensible change in 30″; it would be reduced to nothing by taking the mean of two consecutive observations, and the result would be independent of it. For, let us suppose the first watch to be the quantity r before the second, and put p for the time in which the sound is transmitted by the solid body. When the first observer strikes on his watch at 0″, the other reads on his $0'' - r$; and consequently $p - r$ indicates, before or after 0″, the time at which he hears the

the velocity was measured directly.

the

sound. On the other hand, when the second observer strikes at 30″, the first observer reads 30″ + r; and consequently $p + r$ indicates, beyond 30″, the time in which the sound is transmitted to him. The quantities $p - r$ and $p + r$ therefore are given by these isochronous observations; and half their sum immediately shows the time of transmission p, independant of the differences between the watches, and more exactly than by direct observation.

In the experiments I made, the series of the quantities $p - r$ and $p + r$ were as in the following table.

	$p - r$	$p + r$	Sum, or value of $2p$.
1st series, from 0^h 52′ to 0^h 59′	−2″	+ 2·5″	0·5″
	2	2·5	0·5
	2	2·5	0·5
	2	2·5	0·5
	2	2·5	0·5
	2	2·5	0·5
	2	2·5	0·5
	2	2·5	0·5
2d series, from 1^h 27′ to 1^h 32′	2·8	3·5	0·7
	2·9	3·5	0·6
	3	3·5	0·5
	2·9	3·5	0·6
	3	3·5	0·5
	3	3·5	0·5
	3	3·5	0·5
	2·9	3·5	0·6
	3	3·5	0·5
	3	3·5	0·5
	3·1	3·4	0·4
	Mean value of $2p$.	....	0·52
	Value of p	....	0·26

This differs only 0·03″ from what we found above from the difference of the transmissions: but the last method, as it gives double the quantity to be deduced, deserves the preference. This nearly agrees with the last calculation.

If we add 0·26″, the time of transmission through the solid,

o the difference 2·5″ constantly observed between the of the two sounds, we shall have the whole time of nsmission through the air equal to 2·76″. This time, ited from the length of the pipe, would have been as has just been seen; and the agreement between iumbers, which differ only 0·03″, appears calculated ire some confidence in the results.

Velocity in cast iron more than 10 times as great as in air.

time of transmission through the metal being 0·26″, hat through air is 2·79″, it follows, that the transmis- sound through cast iron is 10·5 times as quick as h air. If this estimation be not sufficiently exact t ratio of the velocities, it is a t at kind this ratio is, and what i to

Other phenomena observed.

In ese e s we had an opportunity of observ l ph ena worthy of remark with respec the power w which sounds, even the faintest, are served and transmitted in tubes, to distances at whi we could scarcely suppose they would be perceptible.

Conversation easy through a pipe of 215 yards.

In our first experiments at the distance of 197 met. [215 yds.] we heard each other so well through the length of the pipe, that it was an inconvenience in the commencement, as the slightest noise was transmitted from one extremity to the other. It was not necessary to speak into the pipe to be heard, as common conversation two yards from the end was transmitted through it clearly; and in writing down my observations I asked Mr. Martin what it was o'clock by his watch, as I would have done a person only two paces from me. This mode of conversing with an invisible neighbour is so singular, that we cannot avoid being surprised, even though acquainted with the cause.

Speaking loud heard 431 yards.

In the experiments made by Messrs. Malus and Bouvard at the distance of 395 m. [431 yds.] they still heard each other, but with much more difficulty. It was necessary to speak very loud, and frequently to desire a repetition of what had been said. Finally, in the last experiment, which we tried at first together on a total length of 951 m. [1040 yds.], the voice was scarcely to be heard when shouting as loud as possible. The sounds of the bell and of the stroke of the hammer were no longer audible through the air.

At 1040 yds. loud shouting scarcely audible, and the sound of the bell or the hammer not

air. The sound through the metal alone was perceptibly transmitted. Lastly, though we could still hear the sound of the voice, it was not sufficiently clear for us to distinguish words, or to transmit the necessary information after our observations. From the great difficulty, which Messrs. Malus and Bouvard had already experienced at a much shorter distance, we were all inclined to suppose, that we had attained a distance, at which the human voice, even the loudest, ceases to be distinguishable in pipes.

at all through the air.

However, the extreme facility with which we heard each other at 200 metres seemed to me to render so great a diminution altogether inexplicable. Besides, in the mathematical theory of the motion of air we find nothing to indicate, that sound should be diminished in cylindrical pipes. It appears on the contrary, that it ought to be transmitted to an indefinite distance with the same intensity, deducting merely the diminution, that the friction of the air against the pipe might perhaps produce. To decide the question, and know positively whether sound were weakened in such an extraordinary degree, I resolved to remove or diminish all the causes of foreign and neighbouring noises, that might drown the sound I sought to hear. I went to the place of experiment only with Mr. Martin and two intelligent workmen, and chose for these experiments the stillest hours of the night, those from one to four in the morning.

But this improbable from the 1st experiments, and from theory.

The experiments repeated in the dead of night,

I then discovered, that my conjectures were well founded. We not only heard the two sounds of the hammer and bell so distinctly as to observe the intervals such as I have reported them; but even the lowest voice was heard so as perfectly to distinguish the words, and to keep up a conversation on all the objects of the experiments. I wished to determine the point at which the voice ceases to be audible, but could not accomplish it: words spoken as low as when we whisper a secret in another's ear were heard and understood; so that not to be heard there was but one resource, that of not speaking at all.

when not only the sounds of the bell and hammer, but the lowest whisper was heard.

From this experiment there can be no doubt, that words may be transmitted so as to be distinctly heard at a more considerable distance. Between a question and answer the interval was not greater, than was necessary for the transmission

...on of sound. For Mr. Martin and me, at the distance of 951 m. [1040 yds.], this time was about 5·58."

Grave and acute sounds have equal velocity.

We also ascertained anew, that grave and acute sounds are transmitted with equal velocity, which is agreeable to theory, and has been several times observed.

Playing on the flute.

Tunes on the flute, played at one extremity of the pipe, were transmitted to the other without any alteration in the intervals of the different intonations. It appeared to me only, that the very high notes were not heard so well as the low notes; and sometimes, when they were extremely high, I lost them entirely; though I heard others that were lower, which, from the nature of the tune, I knew to be weaker than the former*.

Echo of the voice returned repeatedly to the speaker,

I also observed, that, in speaking through the pipe, I heard my own voice repeated by several echoes, which succeeded each other at exactly equal intervals. In our last experiment I counted no less than six, about 0·5″ distant from each other. The last returned after a little less than 3″; that is, in the time requisite for the transmission of the sound to the other end of the pipe. These phenomena occurred equally at each extremity of the pipe, when we spoke into it. Of this I satisfied myself by requesting Mr. Martin, through the pipe, to observe them, without communicating to him my results: and his, which he reported to me immediately in the same way, were perfectly similar. The number of echoes and their intervals were the same, and the total of the time was the same also;

but the sound at the other end single

but the person who is spoken to never hears but one sound.

Detonations.

Lastly, detonations capable of producing a considerable agitation in the air were transmitted to the other end of the pipe with an intensity proportional to their strength.

A pistol fired at one end blew out a candle at the other.

Reports of a pistol fired at one end occasioned a considerable explosion at the other. The air was driven out of the pipe

* Since this paper was read, I have found, that the person who played the flute, having very weak lungs, could with difficulty bring out the high notes, and was frequently obliged to skip them entirely. It was very natural therefore, that I should not hear them: but I have thought proper to let my first account remain, that the reader may see I reported faithfully the smallest particulars; and that my veracity in this circumstance may confirm the other results I observed.

with

with sufficient force to give the hand a smart blow, to drive light substances out of it to the distance of half a yard, and to extinguish a candle, though it was 950 m. [1039 yds.] distant from the place where the pistol was fired.

V.

Observations and Experiments on Pus. By GEORGE PEARSON, *M. D. F. R. S.*

(Concluded from p. 27.)

(Concluded from p. 27.)

SECTION VII. *Conclusions.*

THE statement of the properties of pus in the foregoing inquiry I hope will be found to be true; and I submit to the judgment of others whether or no the following inferences are legitimately established.

General conclusions.

1. That this fluid essentially consists of three distinct substances, viz. 1. An animal oxide, which, among other properties, is distinguished by its being white, opaque, smooth, of the form of fine curdy particles in water; not dissoluble in less than 1000 cold waters; not coagulable into one mass like serum of blood by caloric, alcohol, &c.; only rendered more curdy by water from 160° to 170°; but readily diffusible. —2. A limpid fluid resembling serum of blood in its impregnations, and in its coagulability by caloric, alcohol, &c.; in which the opaque oxide is diffusible but not dissoluble, and which is specifically lighter than that oxide.—3. Innumerable spherical particles visible only by the microscope in this opaque oxide, and in small number in the limpid fluid; not coagulable by any temperature to which hitherto exposed, and not destructible by many things which combine or destroy the opaque oxide; and these globules are specifically heavier than water*.

Pus consists of three distinct substances.

* My obligingly attentive pupils, Mr. BURTON, and Mr. STANSFIELD, house-surgeons of the Lock hospital, collected for me a sufficient quantity of gonorrheal matter to determine, that it consisted of the three ingredients here stated.

2. That

Visible curdy masses.

That the *visible* curdy masses, as well as the fibrous eafy parts, almost always contained in smaller or larger quantities in pus, may be considered as self-coagulated lymph, which in its fluid state is secreted without having the state of aggregation produced in it like that of the *essential* opaque oxide of pus.—Sect. VII, 1.

Red or dark colour of pus.

3. That the reddish, the blackish, and the dark brown colour of pus depends upon the red part of the blood effused or secreted from the same vessels, or from contiguous ones which secrete pus.

Irregular masses.

4. That on some occasions the clotty and irregularly figured masses found in the pus may depend upon disorganization or breach of the contiguous solid parts.

Fœtor.

5. That whenever pus is fœtid to the smell, a portion of it is in the state of putrefactive fermentation, which may be removed by ablutions with water.

Adventitious contagious matters.

6. That there are certain adventitious matters liable to be contained in pus not hitherto rendered palpable to the senses, but known by their effects in exciting contagious diseases; such as small-pox, syphilis, &c. These matters are produced by a specific action in the secretory organs of pus, by such matters themselves either contained in the circulating blood, or on the secreting surface.

Secretion of pus from the blood.

7. That the *essential* substances of which pus consists, as well as some of the adventitious ones (Sect. VII, 1, 2, 3, 6), are separated from the blood by a peculiar organization belonging, or attached to the blood-vessels: which organs of separation or secretion are not only excited to the action which produces pus in deseased states, but they are evidently influenced by the states of other distant organs of the animal œconomy; hence many varieties in the properties of the purulent matter.

Sources of the differences of pus.

8. That the varieties of purulent matter relate to differences of *quantity*—the proportion of the essential substances (1)—and the adventitious parts (2, 3, 4, 5, 6,). The *cream-like* pus consisting of almost purely the opaque oxide and limpid liquid (I, 1, 2,). The *curdy* containing a large proportion of coagulated lymph, or broken down solids. The *serous* abounding in limpid fluid. The *viscid* depending upon

upon the coagulation, and perhaps, inspissation, by union of neutral salts with the opaque oxide.

9. That as the essential parts are secreted in a limpid state, but presently become opaque, owing to a large proportion spontaneously coagulating, and thus becoming the opaque oxide, mixed with the serous liquid, and innumerable spherical particles (Sect. VII, I, 1, 2, 3), it seems reasonable to infer, that these matters are the self-coagulated lymph of the blood and serum, separated by the secretory organs; which act of secretion determines the subsequent state of aggregation of pus, and the globules are at the same time formed analogously to their formation by other secretory organs. How far they are those of the blood altered by secretion may be determined hereafter. It is a collateral proof of this inference, that very thick pus affords from one sixth to one seventh of exsiccated brittle residue, which, as I have found, is nearly the same proportion afforded on the exsiccation of the buffy coat of inflamed blood; while very thin pus affords on exsiccation from one eighth to one eleventh of brittle residue, which is the proportion to be expected from a mixture of serum of blood and self-coagulated lymph, as I have ascertained.

Self-coagulated lymph of the blood and serum separated by secretion.

10. That the constant inpregnating saline and earthy ingredients of pus are dissolved in the serous fluid; and are all separable along with the serum, by ablutions with water, from the opaque oxide (1), except a portion of the phosphate of lime. These impregnations are the same as those of serum of blood, and of expectorated mucous matter, *viz.* muriate of soda; potash neutralized by animal matter or a destructaible acid; phosphate of lime; ammonia neutralized probably by phosphoric acid; with a sulphate, and traces of some other matters mentioned in my former paper. The proportion of these impregnating substances is as the proportion of limpid or serous coagulable fluid, and of course inversely as the proportion of the opaque oxide of pus; but it varies in different cases in given proportions of this oxide, and the limpid fluid. In general, if not always, a given quantity of pus contains a smaller proportion of saline matters than an equal given quantity of expectorated mucous matter, but a given quantity of the limpid coagulated fluid

Saline and earthy ingredients.

 contains

contains a greater proportion of saline matters than an equal given quantity of serum of blood. Hence the thicker the pus the less irritation to the sore which secretes it, and commonly the less the inflammatory or other action of the secreting surface. In different cases, however, the proportion of impregnating saline substances to one another is liable to vary, especially that of phosphate of lime; hence, though rarely, calculi occur of this substance in the cavity of the abscess*. Hence too the exsiccated pus is liable to become soft and moist, from the proportion of neutralized potash being greater than usual; and even deliquescence sometimes occurs of the exsiccated limpid fluid.

Calculi in abscesses.

12. That the same organs, according to their different states, secrete from the blood merely water impregnated with the saline substances of the serum of blood; also this fluid containing various proportions of coagulable matter like that of serum of blood; and serous fluid with self-coagulable lymph, which affords curdy masses: likewise this serous fluid, together with this matter which coagulates of itself after secretion, highly impregnated with invisibly small particles, in such a state of aggregation, as to constitute the thick opaque fluid called pus—which states of the secretory organs are generally attended with inflammatory action, but frequently also without any symptoms of such action.

Different secretions from the same organs in different states.

13. That beside the consistence of pus depending upon the proportion of serous limpid liquid, and opaque matter, it also probably depends upon the mode and state of coagulation of the matter which affords this opaque part; analogously to the different states of consistence of the coagulated blood itself, according to the different conditions of the animal œconomy.

Consistence of pus.

According to the above inferences, I trust, a distinct and definite notion of the substance to be considered as pus is

Distinction of pus from other matters.

* On examining the lungs of a patient who died of pulmonary consumption, concretions were found in a large vomica from the size of mustard seed to a pepper corn, which Dr. E. N. Bancroft reserved for my inquiry. I found they consisted chiefly of phosphate of lime, with an unusually small proportion of animal matter. In another patient of Dr. Nevinson, matter was coughed up, consisting chiefly of phosphate of lime and animal matter, nearly one of the former to three of the latter.

Stones in the lungs.

exhibited

exhibited; and I do not comment on the different results of experiment and conclusions of other writers, because future observers only can determine the truth. What is and what is not pus will now readily be acertained by a few easy experiments; by the obvious properties; and by the consideration of the source of the matter in question: provided, however, that it be unmixed with certain other matters, by which disguise is produced. As already observed it is in pulmonic diseases that the ambiguity occurs; and physicians lay very considerable stress upon the nature of expectorated matter in their practice and reasoning; I shall therefore endeavour to elucidate the subject by remarks on the puriform matter expectorated in different cases.

Puriform matter expectorated

1. An abscess occasioned by acute inflammation not only of a pleurisy, and peripneumony, but of other diseases which have not the symptoms of any one which has received a designation. Here there ought to be no doubt; for the matter which is coughed up suddenly and abundantly on the bursting of the abscess is evidently pus with little mucus. Such matter consists of the essential ingredients of pus, (Sect. VII, 1,) with generally the adventitious substances, (Sect. VII, 2, 3, 4,) *viz.* coagulated lymph, menbranous or fibrous parts, and a small proportion of the red part of blood.

from an abscess after acute inflammation;

2. Purulent expectoration from the rupture of abscesses, or vomicæ of suppurated tubercles. In such cases there has been a chronical cough with viscid sputum, commonly in persons of an advanced age. After this long continued disease, an abundant expectoration of quite a different kind from the former suddenly comes on; by which the patient often dies very speedily; sometimes immediately, being seemingly choaked. This kind of matter evidently consists chiefly of the essential ingredients of pus (Sect. VII, 1,) with not only the adventitious substances, *viz.* clots of self-coagulated lymph, and sometimes the red part of blood, but also masses, which are apparently the broken down solid parts, the cellular membrane, the vessels, and substance of the tubercles, in a disorganized state. The sufferer often says, such matter tastes sweet. The mucus is here in too small a proportion, and not intimately mixed, to occasion disguise.

from the rupture of abscesses of suppurated tubercles;

3. In the bronchitis, or inflammatory affection of the air tubes

in inflam-

matory affection of the air-tubes.

tunes, the membrane remaining entire, attending various diseases, e. g. the measles, a fever with a cold, various continued fevers, an expectoration of thin creamlike matter occurs, at first gradually, but at last in great quantities, continuing for a week or more. Although mucus is usually coughed up with this puriform substance, the two things generally remain in distinctly large masses. With little skill the opaque or puriform fluid may be collected separately from the mucous matter. It will be found to consist almost purely of the three essential constituents of pus (Sect, VII, 1,) there being seldom any adventitious substances.

Muco-purulent matter.

4. Muco-perulent, or commixed expectorated matter. This kind is perhaps of the most frequent occurrence. It is that which many physicians know not how to designate, some consider it to be pus, and others to be mucous matter. This contrariety of opinion arises from the want of definite notions of pus and mucus. Hence the parties are not able to perceive, that in this kind of sputum exist many of the properties of pus, and also of mucus. I have described it in my former paper on expectorated matter. Phil. Trans. 1809, P. II, p. 317*, under the denomination of *opaque ropy matter*, the third kind. I feel no degradation in finding it necessary to confess, that a better acquaintance with the properties of pus has taught me, that I was in an errour, in considering this kind of expectorated matter to differ from other sorts merely in the proportion, and not in the kinds, of constituent parts. It now appears that the sputum in question possesses such properties as might be predicted to exist, from the known properties of pus and mucus separately, in case these two substances should be intimately commixed. Accordingly, the opacity; the straw colour; the greater density than mucus; the great globularity under the microscope; the greater proportion of residue on evaporation to dryness, than from mucus; the milky liquid on heating this matter; the milkiness on agitation in cold water; are properties of pus. But the great viscidity, yet not increased by neutral salts; the less opacity than pus; the less globularity than pus; the smaller porprotion of exsiccated residue than

* Journal, vol. XXV, p. 219.

from

from pus; the moisture, or greater moisture on the exposure of the brittle residue to air, than from that of pus; the more difficult diffusibility through cold water, and less degree of milkiness than from pus: the great proportion of leafy or fibrous masses on agitation in a very large quantity of cold water; the speedy putrescency; are properties of mucus. The mode of coagulation by caloric at 160° and upwards is such as might be expected from the commixture, *viz*: in large masses of curd in a milky liquid, instead of into one uniform mass like pus, or into small curdy masses in a very large proportion of a whey coloured liquid, like mucous sputum. Thick pus affords on evaporation to brittleness, $\frac{1}{7}$ or $\frac{1}{8}$ residue; and transparent sputum of the consistence of jelly, gives about $\frac{1}{18}$ or $\frac{1}{20}$ of such residue: but this opaque matter under inquiry, affords $\frac{1}{10}$ or $\frac{1}{12}$ of brittle residue, according to the proportion of the two substances. I could not separate the supposed pus and mucus from one another, to exhibit them distinctly by water, or by any other means, on account, as I conceive, of the intimate diffusion through one another, and their mutual cohesion. But on evaporating the milky water, produced by agitating this sputum in it, or by letting it stand to collect the sediment, little else besides a mere congeries of globules seen under the microscope was thus obtained. For the same reason, on standing, a serous liquid like that of pus (Sect. VII, 1) does not separate, or only partially, from the opaque part, so as to render it possible by ablution, to collect this coagulable liquid like that of pus: and the greater proportion of water, belonging to the mucus, occasions the coagulation by caloric, to afford only a milky liquid, instead of a uniform mass of curd.

This kind of sputum, consistently with the phenomena, must be produced by secretion from the bronchial membrane in its entire state, and not by ulceration or abscess. For it is secreted in many cases, at the rate of a pint or more in each 24 hours, for weeks and months successively, and for 20 or more successive winters. Also many persons recover their good health after this secretion, and it is the usual termination favourably of pneumonia, bronchitis, &c. It is produced by any disease of great irritation of the lungs; as I have found from

From secretion without ulceration.

Broken w[illegible] n ossification of the bronchial or pulmonary arteries: from calculi: from broken wind, or rupture of air cells, &c.*

Secreted in other cases, and from the nose.

It is secreted also in consequence of irritation of the bronchial membrane by tubercles, vomicæ, water in the cavities of the chest, &c. The same kind of matter is secreted from the nose on the decline of a common severe coryza in many cases.

Sometimes indicates death, sometimes recovery.

It appears then, that this kind of matter is a symptom of the most fatal, as well as harmless diseases—it is a symptom in one case of the progress of disease to death, and in another of the termination in health, by being seemingly a critical discharge. Perhaps, if these facts had been observed and considered, numerous mistakes in prognostics would have been avoided, and better practice have been employed; because the nature of diseases would have been rightly understood. From this representation it is plain, that a just opinion cannot be given merely from the examination of the sputum, without considering the disease by which it is produced, or of which it is a symptom.

The proportion must also be considered of the pus and mucus in sputum: it may be estimated, by attending to the properties of each, as above stated.

Secretion of muco-purulent matter.

Such a compound as the present scarcely is produced in any other part, but in the bronchial, and mucous membrane of the nose, because of the abundant secretion of mucus from these membranes. And when it is conceived, that both pus and mucus are secreted in a limpid state, from the same or at least contiguous organs, where they first intimately commix, and then become inspissated; it will appear reasonable, that they cannot be readily, or at all completely separated again from one another. There is indeed, in these cases, no necessity for the admission of the secretion of the limpid fluid of pus of abscesses (Sect. VII, 1); for it appears to me not unjust to consider mucus to be nothing more than the serum of blood, altered in its composition and proportion of water, so as to produce a viscid texture. The secretory organs of the mucous membrane, by virtue of their peculiar power, separate from the blood, in health,

* I believe this state of the lungs to have been first ascertained in broken winded horses, by Mr. Colman.

the

the mucus as above said, with some globules, and also a small proportion of the self-coagulable lymph; which appears, on agitating mucus in a large proportion of cold water, in the form of leafy and fibrous masses*. The same secretory organs, it is easily conceivable, may, in a diseased state, be excited to separate also self-coagulable matter from the blood, with more globules, in such a state as to become pus. Hence, such a commixture of the two substances must correspond to the opaque, viscid, expectorated sputum, of which I am writing.

If I thought farther reasoning proper, it would be manifest, that all the phenomena, both in health and disease, belonging to the various kinds of sputum, consist with the theory above delivered.

VI.

Description of a Tachometer, or an Instrument to ascertain the Velocities of Machinery: by Mr. BRYAN DONKIN, *of Fort Place, Bermondsey*†.

IN the employment of machinery it is evidently of great importance to be provided with an easy and ready method for discovering at all times, whether the motion of the machine is quicker or slower than what is known to be best adapted for the object in view. This advantage, it is hoped, may be derived from the tachometer; for it is an instrument which requires only to be adjusted once for all to any particular machine, and then it will always be ready without the help of calculation or of a time-piece, to indicate instantly upon inspection the slightest excess or defect in the actual velocity.

Advantageous to ascertain the velocity of machinery.

A front view of the tachometer is represented in fig. 1, and a side view in fig. 2, of Pl. III. X Y Z, fig. 1, is the vertical section of a wooden cup, made of box, which is drawn

An instrument for this purpose described.

* Serum of blood appears always to contain self-coagulable lymph, which is deposited on standing; and this appearance led Gaber, Pringle, and Cullen, into the erroneous opinion of this deposit being pus itself.

† Trans. of the Soc. of Arts, vol. XXVIII, p. 185. The gold medal was voted to Mr. Donkin for this invention.

drawn in elevation at X, fig. 2. The whiter parts of the section, in fig. 1, represent what is solid, and the dark parts what is hollow. This cup is filled with mercury up to the level L L, fig. 1. Into the mercury is immersed the lower part of the upright glass tube A B, which is filled with coloured spirits of wine, and open at both ends, so that some of the mercury in the cup enters at the lower orifice, and when every thing is at rest, supports a long column of spirits, as represented in the figure. The bottom of the cup is fastened by a screw to a short vertical spindle D, so that when the spindle is whirled round, the cup, (the figure of which is a solid of revolution) revolves at the same time round its axis, which coincides with that of the spindle.

In consequence of this rotation, the mercury in the cup acquires a centrifugal force, by which its particles are thrown outwards, and that with the greater intensity, according as they are more distant from the axis, and according as the angular velocity is greater. Hence, on account of its fluidity, the mercury rises higher and higher as it recedes from the axis, and consequently sinks in the middle of the cup; this elevation at the sides, and consequent depression in the middle, increasing always with the velocity of rotation. Now the mercury in the tube, though it does not revolve with the cup, cannot continue higher than the mercury immediately surrounding it, nor indeed so high, on account of the superincumbent column of spirits. Thus the mercury in the tube will sink, and consequently the spirits also; but as that part of the tube which is within the cup is much wider than the part above it, the depression of the spirits will be much greater than that of the mercury, being in the same proportion in which the square of the larger diameter exceeds the square of the smaller.

Method of using it.

Let us now suppose, that, by means of a cord passing round a small pulley F, and the wheel G, or H, or in any other convenient way, the spindle D is connected with the machine, the velocity of which is to be ascertained. In forming this connection, we must be careful to arrange matters so, that, when the machine is moving at its quickest rate, the angular velocity of the cup shall not be so great

as

as to depress the spirits below C into the wider part of the tube. We are also, as in the figure, to have a scale of inches and tenths applied to A C, the upper and narrower part of the tube, the numeration being carried downward from zero, which is to be placed at the point to which the column of spirits rises when the cup is at rest.

Then the instrument will be adjusted, if we mark on the scale the point to which the column of spirits is depressed, when the machine is moving with the velocity required. But, as in many cases, and particularly in steam-engines, there is a continued oscillation of velocity, in these cases we have to note the two points between which the column oscillates during the most advantageous movement of the machine.

Here it is proper to observe, that the height of the column of spirits will vary with the temperature, when other circumstances are the same. On this account the scale ought to be movable; so that, by slipping it upwards or downwards, the zero may be placed at the point to which the column reaches when the cup is at rest; and thus the instrument may be adjusted to the particular temperature with the utmost facility, and with sufficient precision. The essential parts of the tachometer have now been mentioned, as well as the method of adjustment; but certain circumstances remain to be stated. Correction for temperature.

The form of the cup is adapted to render a smaller quantity of mercury sufficient, than what must have been employed either with a cylindrical or hemispherical vessel. In every case two precautions are necessary to be observed: Precautions. First, That, when the cup is revolving with its greatest velocity, the mercury in the middle shall not sink so low as to allow any of the spirits in the tube to escape from the lower orifice; and that the mercury, when most distant from the axis, shall not be thrown out of the cup. Secondly, That, when the cup is at rest, the mercury shall rise so high above the lower end of the tube, that it may support a column of spirits of the proper length.

Now in order that the quantity of mercury, consistent with these conditions, may be reduced to its minimum, it is necessary—first, that if M M, fig. 1, is the level of the mercury

y at the axis when the cup is revolving with the greatest velocity, the upper part M M X Y of the cup should be of such a form, as to have the sides covered only with a thin film of the fluid; and secondly, that for the purpose of raising the small quantity of mercury to the level L L, which may support a proper height of spirits when the cup is at rest; the cavity of the cup should be in a great measure occupied by the block K K, having a cylindrical perforation in the middle of it for the immersion of the tube, and leaving sufficient room within and around it for the mercury to move freely both along the sides of the tube and of the vessel.

The block K K is preserved in its proper position in the cup or vessel X Y Z, by means of three narrow projecting slips or ribs placed at equal distances round it, and is kept from rising or floating upon the mercury by two or three small iron or steel pins inserted into the underside of the cover, near the aperture through which the tube passes.

Form of the cup.

It would be extremely difficult, however, nor is it by any means important, to give to the cup the exact form, which would reduce the quantity of mercury to its minimum; but we shall have a sufficient approximation, which may be executed with great precision, if the part of the cup above M M is made a parabolic conoid, the vertex of the generating parabola being at that point of the axis to which the mercury sinks at its lowest depression, and the dimensions of the parabola being determined in the following manner: Let V G, fig. 3, represent the axis of the cup, and V the point, to which the mercury sinks at its lowest depression; at any point G above V, draw G H perpendicular to V G; let n be the number of revolutions, which the cup is to perform in 1″ at its quickest motion; let v be the number of inches, which a body would describe uniformly in 1″, with the velocity acquired in falling from rest, through a height $=$ to G V; and make G H $= \frac{v}{3\ 14\ n}$. Then, the parabola to be determined is that which has v for its vertex, V G for its axis, and G H for its ordinate at G. The cup has a lid to prevent the mercury from being thrown out of it, an event which would take place with a very moderate velocity of

of rotation, unless the sides were raised to an inconvenient height; but the lid, by obstructing the elevation at the sides of the cup, will diminish the depression in the middle, and consequently the depression of spirits in the tube: on this account a cavity is formed in the block immediately above the level L L, where the mercury stands when the cup is at rest; and thus a receptacle is given to the fluid which would otherwise disturb the centrifugal force, and impair the sensibility of the instrument.

Curve at the bottom of the tube.

It will be observed, that the lower orifice of the tube is turned upwards. By this means, after the tube has been filled with spirits by suction, and its upper orifice stopped with the finger, it may easily be conveyed to the cup and immersed in the quicksilver without any danger of the spirits escaping, a circumstance which otherwise it would be be extremely difficult to prevent, since no part of the tube can be made capillary, consistently with that free passage to the fluids, which is essentially necessary to the operation of the instrument.

Method of setting the instrument in motion.

We have next to attend to the method of putting the tachometer in motion, whenever we wish to examine the velocity of the machine. The pulley F, which is continually whirling during the motion of the machine, has no connection whatever with the cup, so long as the lever Q R is left to itself. But when this lever is raised, the hollow cone T, which is attached to the pulley and whirls along with it, is also raised, and embracing a solid cone on the spindle of the cup, communicates the rotation by friction. When our observation is made, we have only to allow the lever to drop by its own weight, and the two cones will be disengaged, and the cup remain at rest.

The lever Q R is connected by a vertical rod to another lever S, having at the extremity S a valve, which, when the lever Q R is raised, and the tachometer is in motion, is lifted up from the top of the tube, so as to admit the external air upon the depression of the spirits; on the other hand, when the lever Q R falls, and the cup is at rest, the valve at S closes the tube, and prevents the spirits from being wasted by evaporation.

Increase of the

It is lastly to be remarked, that both the sensibility and the

sensibility and range of the instrument.

the range of the instrument may be infinitely increased; for, on the one hand, by enlarging the proportion between the diameters of the wide and narrow parts of the tube, we enlarge in a much higher proportion the extent of scale corresponding to any given variation of velocity: and on the other hand, by deepening the cup so as to admit when it is at rest a greater height of mercury above the lower end of the tube, we lengthen the column of spirits which the mercury can support, and consequently enlarge the velocity, which, with any given sensibility of the instrument, is requisite to depress the spirits to the bottom of the scale.

Applicable to delicate experiments.

Hence the tachometer is capable of being employed in very delicate philosophical experiments, more especially as a scale might be applied to it, indicating equal increments of velocity. But in the present account it is merely intended to state how it may be adapted to detect in machinery every deviation from the most advantageous movement.

VII.

A Mode of conveying Intelligence from a reconnoitring Party. In a Letter from a Correspondent:

To W. NICHOLSON, Esq.

SIR,

Mode of conveying intelligence by a reconnoitring party.

I Herewith send you a model, which I denominate a *Hypograph*, and which appears to me likely to be of use in the march of troops, &c.

It may consist of any number of men and officers, but I conceive an officer and six men quite sufficient. The use it seems most adapted to is, when a mountain or high ground is in front, and it is wished by the commanding officer to know what may be on the other side, by dispatching such a number of men intelligence can be at once conveyed by changing the front of one or more men to express numbers, or permanent signals, as agreed on, as the boards of a telegraph; and by the officer placing himself on either flank, centre, or rear, the numbers would be quadrupled. I know by

by experience it may be distinguished at a great distance. Should you think this worthy of notice, it will be a satisfaction to,

Sir, your obedient servant,

August, 1811. H. I. B.

The model consists of little tin casts of six horse soldiers and one officer, see Pl. III, fig. 4. These are placed on a slip of wood, and each is movable on a pivot, so that it may be turned into any position.

VIII.

Description of a Machine for separating Iron Filings from their Mixture with other Metals: by Mr. J. D. Ross, *Princes Street, Soho**.

SIR,

I Hope you will be pleased to lay before the gentlemen of the Society of Arts &c. the model of a machine, which I have invented to separate iron-filings, turnings, &c., from those of brass or finer metals, in place of the slow and tedious process hitherto employed, which is by a common magnet held in the hand. By my invention many magnets may now be employed at once, combined and attached to a machine on a large scale. The magnetic hammers are so contrived as to take up the iron-filings from the mixture of them with other filings, or metallic particles, placed in the trays or end boxes, and drop them into the receiving box in the centre, which is effected by the alternate motion of a winch-handle, working the two magnetic hammers placed at two angles of a quadrant or anchor. In proportion to the power of the magnets, and to the force of the blow given by the hammers, a great quantity of iron is separated from the brass, by the alternate motion, and dropped into the receiver placed in the centre of the machine. **Machine for separating iron filings from other matters.**

I have shown the model to persons engaged in various

* Trans. of the Soc. of Arts, &c., vol. XXVIII, p. 206. Five guineas were voted to Mr. Ross for this invention.

metallic

c works*, who give me great encouragement by their atures and sanction, and I hope it will meet with the ...iety's approbation.

I am, Sir,

Your most obedient and humble servant,

J. D. ROSS.

Reference to Mr. Ross's *Machine for separating Iron Filings from those of Brass, or other Metals*, Figs. 1 *and* 2, Pl. IV.

Description of the machine.

A is an axis of brass, and B a handle upon the end of it: C is a piece of brass in form of an anchor, at each end of which a horse-shoe magnet is fixed, in the manner shown at fig. 1, where *c* is the arch of the anchor, and *d* a piece of brass having a hole through it to receive the legs *e e* of the magnet, which is fixed to the arch by a screw *f*, tapped into the arch. The anchor is mounted upon the pivots of the axis A, in a frame E, which encloses it; on the outside of the frame are two blocks of wood, FF, in each of which a hollow or tray is formed to receive the filings which are to be separated from the iron they contain in these hollows.

Its mode of operation.

The magnets fixed at the ends of the anchor strike upon the filings, and select, by the magnetic attraction, all the iron among them; the anchor is then turned over by the handle B, and the opposite magnet strikes in the other hollow F. At this time the other magnet is just over the axis, and by the jerk of its opposite striking the block F, the iron-filings are shaken off, and fall down on the bottom of the frame, or receiver. In this manner the handle B, being moved backwards and forwards, strikes the magnets alternately in the two blocks F; and at the same time that one strikes, the opposite is cleared from the iron it has picked up by the shock. G is a screen of thin board to prevent the filings being scattered.

* Eleven different persons certified, that they consider Mr. Ross's invention of a machine for separating iron filings, turnings, &c., from those of brass or finer metals, as likely to prove extremely useful in various branches of workers in metal.

IX.

IX.

A new Method of constructing Sash Windows, so as to be cleaned or repaired without the necessity of any Person going on the outside of the House: by G. MARSHALL, *No. 15, Cecil Court, St. Martin's Lane**.

SIR,

IN consequence of the numerous accidents, which occur from cleaning and painting the outside of windows, I beg leave to submit to the inspection of the Society a model of a sash-window, which, if it meets their approbation, and becomes generally adopted, will, I think, save the life of many a fellow-creature; because the present mode of cleaning or painting the outside of windows is generally done by persons leaning out of the window, or getting upon a plank, or some other convenience made for the purpose, and projecting on the outside of the house; hence, from carelessness and inattention, many fatal accidents have occurred, and the services of many persons lost to their families and the public. One instance of this kind happened about three weeks ago to a man, who was standing on a board cleaning the outside of a window, when, the board giving way, as frequently happens, the man was precipitated, and impaled upon the spikes of the iron pales, which enclosed the area below, whence he was conveyed to the hospital with no hopes of recovery. This unhappy man, I was informed, had a large family depending upon him for subsistence. I was so shocked with the circumstance, that I was not easy till I had made the model, which I thought would be the means of preventing similar accidents. This model I beg leave to lay before the Society, and if it should be so fortunate as to meet with their encouragement, I will receive any donation from them with thankfulness, and have no doubt that it will be found to possess many advantages. In appearance it resembles a common sash, and the upper or lower sheet may be moved up and down in a

Accidents from cleaning windows frequent.

Contrivance to prevent this.

* Trans. of the Soc. of Arts, &c., vol. XXVIII, p. 209. Fifteen guineas were voted to Mr Marshall.

similar manner; beside which, by pushing two small springs back in the upper sheet, and at the same time pulling the sash inwards, you may turn the outside of the sash towards you, into the room, so that it may be easily painted, glazed, or cleaned by a person standing within the room, without the necessity of removing the slips or beadings, by doing which, in the common mode, the glass is frequently broken and the beads lost, left loose, or mismatched, and a considerable expense incurred. By turning the lower sash of my invention in a horizontal or inclining direction, you can look into the street without being wet in rainy weather, or the rain driving into the room and damaging the furniture. Old windows may be altered to act upon this principle, at an expense of twelve shillings per window; and new sashes and frames may be thus made for only six shillings more than the common price.

Another advantage.

The expense trifling, either in new or old sashes.

I remain, Sir,

Your obedient humble Servant,

GEORGE MARSHALL.

Reference to the Delineation of Mr. Marshall's Window-Sash, fig. 3, Pl. IV.

Explanation of the plate.

A A represents the window-frame; B B the lower, and C C the upper sash. The frame A A is fitted with grooves, weights, and pullies, in the usual manner; the fillets on the sash, which enter the grooves, are not made in the same piece with the sash-frame, but fastened thereto by pivots about the middle of the sash; upon these pivots the sash can be turned as at C C, so as to get at the outside without disturbing the fillets or grooves; when the sash is placed vertically, as at B B, two spring-catches at *a a* shoot into and take hold of the sliding fillets, so that in this state the sash slides up or down in the usual manner; but it can be immediately released, and turned inside out, by pushing back the springs, and at the same time pulling the sash inwards; this turns the outside towards the room, so that the sash may easily be painted, glazed, or cleaned on the outside by a person within the room, without removing the beads, which confine the sash to slide up and down vertically; in the common way these beads are frequently broken

or

or misplaced, and cause considerable trouble by being always loose. By inclining the sash on its pivots, the highest point being within the room, the window may be left open in the most severe rain without danger of any entering the room, and a person may look out into the street without being wet.

X.

Observations on the peculiar Appearances of those Meteors commonly called Shooting Stars. In a Letter from THOMAS FORSTER, *Esq.*

To W. NICHOLSON, Esq.

SIR,

ONCE more I trouble you with some meteorological observations, which, if you think worthy, I shall be obliged to you to insert in your next. In a former number of your Journal I noticed an apparent peculiarity in the electric state of the atmosphere, during which the action of Mr. De Luc's aërial electroscope was very irregular. The principal circumstances, which characterised such a state of the atmospherical electricity, were the continual appearance of the *cirrus* cloud, which, like Proteus, was for ever changing its shape, and presenting itself to the eye under new figures; the prevalence of strong easterly and variable winds; and dry air. Among other circumstances I remarked the appearance of numerous small meteors, or falling stars as they are commonly called, during the night. Peculiarity in the electric state of the atmosphere.

The same kind of weather has returned again this autumn, marked by similar circumstances, and the small meteors have again been numerous. On this last circumstance I dwell particularly; for I have observed, that these meteors vary very considerably in appearance according to the kind of weather which prevails. Those which I have alluded to, and which are usually seen during the prevalence of clear *dry* weather and easterly winds, are small, they shoot along very rapidly, and leave little or no train behind them; they have so much the colour and general appearance of the stars, that they have hence received their vulgar appellation. Similar Similar appearances again recurring. Shooting stars have different appearances.

milar to these are those which are common in clear frosty winter nights. Larger ones than these generally attend warm summer evenings, particularly when *cirro-cumulus* and thunder clouds abound*, with easterly winds. On the 10th of last month, a showery day with northerly wind was followed by a very clear night abounding with small meteors, but they were of a very peculiar and unusual kind, being of a blueish white colour, like the burning of phosphorus, and they left long trains behind them, of the same colour, which lasted for two or three seconds after their extinction. I suppose in the space of an hour I saw above thirty of them, but they were all of this kind, and left the long white tails, which remained for some seconds in the tract in which the stars had gone.

Some of a peculiar appearance noticed.

These kind of meteors are strikingly different from the common kind noticed above; I have sometimes seen them before, but it has always been in the clear intervals of *showery* weather, previous to the occurrence of *high wind*: it was probably this sort of meteor to which Virgil alluded as a prognostic of windy weather.

These have been seen only in the clear intervals of showery weather, followed by high winds.

Alluded to by Virgil.

Sæpè etiam stellas, vento impendente, videbis
Præcipites cœlo labi, noctisque per umbram
Flammarum longcs a tergo albescere tractus.

Georg. lib. i, v. 366.

A stationary meteor.

On the evening of the 25th of last June I saw a meteor, which was a perfectly stationary accension, and lasted scarcely a second; it was followed by many days of damp rainy weather.

I wish that Meteorologists would note down the peculiarities observable in meteors in their monthly journals.

I shall conclude by observing, that, if these considerations should appear trifling and frivolous to any of your readers, it must be remembered, that it is only by accurate and repeated observation of a multitnde of phenomena, that the science of meteorology can be brought to any degree of perfection.

I remain, Sir, yours &c.

Clapton, Sept. the 18th, 1811. THOMAS FORSTER.

* I do not allude to those very large meteors, which occasionally appear: Such for example, as that seen in August, 1783.

XI.

XI.

On the Composition of Zeolite. By JAMES SMITHSON, *Esq.* *F. R. S**.

MINERAL bodies being, in fact, *native chemical preparations*, perfectly analogous to those of the laboratory of art, it is only by chemical means, that their species can be ascertained with any degree of certainty, especially under all the variations of mechanical state and intimate admixture with each other, to which they are subject. Species of minerals can be ascertained only by chemistry.

And accordingly, we see those methods, which profess to supersede the necessity of chemistry in mineralogy, and to decide upon the species of it by other means than hers, yet bring an unavoidable tribute of homage to her superior powers, by turning to her for a solution of the difficulties, which continually arise to them; and to obtain firm grounds to relinquish or adopt the conclusions, to which the principles they employ lead them.

Zeolite and natrolite have been universally admitted to be species distinct from each other, from Mr. Klaproth having discovered a considerable quantity of soda and no lime, in the composition of the latter, while Mr. Vauquelin had not found any portion of either of the fixed alkalis, but a considerable one of lime, in his analysis of zeolite†. Zeolite and natrolite supposed to be distinct species.

The natrolite has been lately met with under a regular crystalline form, and this form appears to be perfectly similar to that of zeolite; but Mr. Haüy has not judged himself warranted by this circumstance, to consider these two bodies as of the same species, because zeolite, he says, "does not contain an atom of soda‡."

I had many years ago found soda in what I considered to be zeolites, which I had collected in the island of Staffa, having formed Glauber's salt by treating them with sulphuric acid; and I have since repeatedly ascertained the presence of the same principle in similar stones from various Soda found in supposed zeolites long ago;

*Phil. Trans. for 1811, p. 171. †Journal des Mines, No. XLIV.
‡Journal des Mines, No. CL, Juin 1809, p. 458.

other

other places; and Dr. Hutton and Dr. Kennedy had likewise detected soda in bodies, to which they gave the name of zeolite.

but their identity with Haüy's mesotype not ascertained.

There was, however, no certainty, that the subjects of any of these experiments were of the same nature as what Mr. Vauquelin had examined, were of that species which Mr. Haüy calls mesotype.

A specimen sent by Mr. Haüy.

Mr. Haüy was so obliging as to send me lately some specimens of minerals. There happened to be among them a cluster of zeolite in rectangular tetrahedral prisms, terminated by obtuse tetrahedral pyramids, the faces of which coincided with those of the prism. These crystals were of a considerable size, and perfectly homogeneous, and labelled by himself "*Mesotype pyramidée du depart. du Puy de Dôme.*" I availed myself of this very favourable opportunity, to ascertain whether the mesotype of Mr. Haüy and natrolite did or did not differ in their composition, and the results of the experiments have been entirely unfavourable to their separation, as the following account of them will show.

This zeolite, or mesotype, analysed.

10 grains of this zeolite being kept red hot for five minutes lost 0·75 of a grain, and became opaque and friable. In a second experiment, 10 grains, being exposed for 10 minutes to a stronger fire, lost 0·95 of a grain, and consolidated into a hard transparent state.

10 grains of this zeolite, which had not been heated, were reduced to a fine powder, and diluted muriatic acid poured upon it. On standing some hours, without any application of heat, the zeolite entirely dissolved, and some hours after, the solution became a jelly: this jelly was evaporated to a dry state, and then made red hot.

Water was repeatedly poured on this ignited matter, till nothing more could be extracted from it. This solution was gently evaporated to a dry state, and this residuum made slightly red hot. It then weighed 3·15 grains. It was *muriate of soda.*

The solution of this muriate of soda, being tried with solutions of carbonate of ammonia and oxalic acid, did not afford the least precipitate, which would have happened had

had the zeolite contained any lime, as the muriate of lime* would not have been decomposed by the ignition.

The remaining matter, from which this muriate of soda had been extracted, was repeatedly digested with marine acid, till all that was soluble was dissolved. What remained was silica, and, after being made red hot, weighed 4·9 grains.

The muriatic solution, which had been decanted off from the silica, was exhaled to a dry state, and the matter left made red hot. It was alumina.

To discover whether any magnesia was contained among this alumina, it was dissolved in sulphuric acid, the solution evaporated to a dry state, and ignited. Water did extract some saline matter from this ignited alumina, but it had not at all the appearance of sulphate of magnesia, and proved to be some sulphate of alumina, which had escaped decomposition, for on an addition of sulphate of ammonia to it, it produced crystals of compound sulphate of alumina and ammonia, in regular octahedrons.

This alum and alumina were again mixed and digested in ammonia, and the whole dried and made red hot. The alumina left weighed 3·1 grains.

Being suspected to contain still some sulphuric acid, this alumina was dissolved in nitric acid, and an excess of acetate of barytes added. A precipitate of sulphate of barytes fell, which after being edulcorated and made red hot, weighed 1·2 grains. If we admit $\frac{1}{3}$ of sulphate of barytes to be sulphuric acid, the quantity of the alumina will be $= 3·1 - 0·4 = 2·7$ grains.

From the experiments of Dr. Marcet†, it appears, that 3·15 grains of muriate of soda afford 1·7 grain of soda.

Hence, according to the foregoing experiments, the 10 grains of zeolite analysed consisted of

Its component parts.

Silica	4·90
Alumina	2·70
Soda	1·70
Ice	0·95
	10·25

* These names are retained for the present, as being familiar, though since Mr. Davy's important discovery of the nature of what was called oxymuriatic acid, the substances, to which they are applied, are known not to be salts, but metallic compounds analogous to oxides.

† Phil. Trans. 1807: or Journal, vol. XX, p. 30.

As these experiments had been undertaken more for t purpose of ascertaining the nature of the component pa of this zeolite than their proportions, the object of them w considered as accomplished, although perfect accuracy the latter respect had not been attained, and which, indee the analysis we possess of natrolite by the illustrious chemi of Berlin renders unnecessary.

Reasons for retaining the name of zeolite.

I am induced to prefer the name of zeolite for this sp cies of stone, to any other name, from an unwillingness obliterate entirely from the nomenclature of mineralog while arbitrary names are retained in it, all trace of one the discoveries of the greatest mineralogist who has yet a peared; and which, at the time it was made, was consider as, and was, a very considerable one, being the first additi of an earthy species, made by scientific means, to those e tablished immemorially by miners and lapidaries, and hen having, with tungsten and nickel, led the way to the gre and brilliant extension, which mineralogy has since r ceived. And, of the several substances, which, from t state of science in his time, certain common qualities i duced Baron Cronstedt to associate together under the nan of zeolite; it is this which has been most immediately u derstood as such, and the qualities of which have been a sumed as the characteristic ones of the species.

Names given by discoverers should not be altered.

Indeed, I think, that the name imposed on a substan by the discoverer of it ought to be held in some degree s cred, and not altered without the most urgent necessity f doing it. It is but a feeble and just retribution of respe for the service, which he has rendered to science.

Existence of phosphoric acid suspected in it, but none found.

Professor Struve, of Lausanne, whose skill in mineralog is well known, having mentioned to me, in one of his letter that, from some experiments of his own, he was led to su pect the existence of phosphoric acid in several stones, an particularly in the zeolite of Auvergne, I have directed m inquiries to this point, but have not found the phosphori or any other acknowledged mineral acid, in this zeolite.

Is quartz an acid?

Many persons, from experiencing much difficulty i comprehending the combination together of the earth have been led to suppose the existence of undiscovere acids in stony crystals. If quartz be itself considered as a acic

acid, to which order of bodies its qualities much more nearly assimilate it, than to the earths, their composition becomes readily intelligible. They will then be neutral salts, silicates, either simple or compound. Zeolite will be a compound salt, a hydrated silicate of alumina and soda, and hence a compound of alumina not very dissimilar to alum. And topaz, the singular ingredients of which, discovered by Mr. Klaproth, have called forth a query from the celebrated Mr. Vauquelin, with regard to the mode of their existence together*, will be likewise a compound salt, consisting of silicate of alumina, and fluate of alumina.

Our acquaintance with the composition of the several mineral substances, is yet far too inaccurate, to render it possible to point out with any degree of certainty the one of which zeolite is a hydrate, however the agreement of the two substances in the nature of their constituent parts, and in their being both electrical by heat, directs conjecture towards tourmaline. Is zeolite a hydrated tourmaline?

St. James's Place, Jan. 22, 1811.

Addition to the Account of native Minium.

After I had communicated to the President the account of the discovery of native minium, printed in the Philosophical Transactions for 1806†, I learned, that this ore came from the lead mines of Breylau in Westphalia.

XII.

Extract from a Paper communicated to the American Philosophical Society on the Discovery of Palladium in a Native Alloy of Gold; by Mr. J. Cloud, *Director of the Chemical Processes at the Mint of the United States*‡.

IN 1807 about 820 ounces of gold bullion were brought into the mint of the United States. They consisted of 120 Gold from Portuguese America.

* Annales du Museum d'Hist. Nat. tome 6, p. 24.

† See Journal, vol. XVI, p. 127.

‡ Annal. de Chim. vol LXXIV, p. 99.

small

ian ingots, each stamped on one side with the arms of Portugal, and the inscription *Rio das montis*, and on the other with a globe. The fineness of each ingot too was marked on it. Among these were two differing from the others so much in colour, that Mr. Cloud preserved one, weighing 3 oz, 11 dwts, 12 grs, to examine it. The following were the experiments he made.

Analysed.

No silver. 1. Nitromuriatic acid was employed on one portion of the ingot, to find whether it contained any silver; and none was discovered.

No metal easily oxidable. 2. Twenty four carats were mixed with 48 carats of fine silver, and cupelled with lead, to separate any oxidable metal that might be present: but there was no diminution of weight, consequently the alloy contained no metal easily oxidable.

Gold. 3. The fine metals of the preceding experiment were flattened between rollers, and subjected to the action of pure nitric acid. The silver and the native alloy mixed with the gold, were dissolved by the acid, which acquired a deep brown red colour. The metal that remained, washed with pure water, and dried by the fire, weighed 22 carats $1\frac{1}{2}$ gr. It had all the appearance of fine gold.

No platina. 4. The metals not dissolved in the latter experiment were subjected to the action of nitromuriatic acid. The whole was dissolved, except a small quantity of silver, which had escaped the action of the nitric acid. The solution was assayed with muriate of ammonia, and other tests, from an expectation of finding platina, but no trace of this metal was discovered. The gold thrown down was pure to $\frac{1}{151}$

Some other metal. 5. Pure muriatic acid was poured into the metallic solution resulting from Exp. 3, till the silver was completely thrown down, and the acid was in considerable excess. None of the colouring matter was precipitated from the solution, which remained red, and did not appear at all changed, notwithstanding the precipitation of the silver.

From these preliminary experiments it appeared, that the alloy was a compound of gold and some metal capable of resisting cupellation, and soluble both in nitric and nitromuriatic acids. In adopting the following mode of analysis

Palladium.

evident

evident proofs of the existence of a metal possessing all the properties of palladium were obtained.

I. The whole ingot was combined with twice its weight of fine silver, and cupelled with lead equal in weight to the mixture. Analysis of the whole.

II. The cupelled metals were reduced to thin plates, and kept in boiling nitric acid till the silver and palladium were dissolved. The deep brown red solution was decanted, and the remaining gold washed with distilled water, which was afterward mixed with the decanted solution.

III. Pure muriatic acid was added to the preceding solution, till it was in excess, and nothing more fell down. The liquid retaining its red colour was decanted off, and the precipitate washed with distilled water. The waters of elutriation were added to the decanted liquor, which then held nothing in solution but palladium.

IV. A solution of pure potash * was poured into the metallic solution of the preceding experiment, till the whole of the palladium was thrown down in a brown flocculent precipitate. This was washed with distilled water, collected on a filter, and dried.

V. A portion of the precipitate obtained in this experiment was put into a crucible without addition, and exposed to a heat of about 60° of Wedgwood; when a metallic button of palladium was obtained of the spec. grav. of 11·041.

VI. Another portion of the precipitate of Exp. IV was mixed with black flux, and exposed to the same degree of heat as in the preceding experiment. The result was the same.

A metal supposed to be palladium, thus obtained from a source where it was not known to exist, required to be compared with the palladium obtained from crude platina, to confirm its identity. Comparative experiments were accordingly made with prussiate of mercury, fresh muriate of tin, and other tests. The metals from these two sources did not exhibit the least difference. The alloy was palladium;

Native gold is never found perfectly pure. Hitherto it has and no other was present.

* Carbonate of potash will not answer so well, because part of the palladium would remain dissolved in the carbonic acid.

ays been seen alloyed with silver or copper, and most
nly with both, and with other metals also. The
at was the subject of the preceding experiments ap-
pea- o have been alloyed with palladium alone. If it had
been lloyed with any other metal, except silver or platina,
the periment No. II would have shown it; and silver
wo have been discovered by the first experiment, and
plat by the fourth.

XIII.

Analysis … *tique Mosaic, found at Rome:* … 'ARCET *.

The cement described. THIS cement is of a yellowish white, very compact, without grains, and pretty hard. It blackens a little in the fire. Before calcination it effervesces briskly: but after it has been calcined, nitrous acid dissolves it without evolving any carbonic acid. In the former case a few yellowish flocks remain, and some fragments of a reddish brown colour, but little compact, and resembling the porous lavas, or puzzolana. The yellowish flocks are destructible in the fire.

Sulphuric acid precipitates nothing from these solutions, therefore they contain no lead.

Ammoniac throws down only a little alumine and oxide of iron.

Analysis of it. 5 gram. [77·29 grs] of this cement, calcined under a muffle for eight hours, no longer effervesced, and weighed only 2·815 gr. [43·48 grs]; which indicates in 100 parts

56·3 of quicklime; and

43·7 of vegetable or animal matter and carbonic acid.

10 grammes of this cement left 4·1 of carbonic acid, when acted on by nitric acid. We have therefore in 100 parts

59 of quicklime and animal or vegetable matter, and

41 of carbonic acid.

* Ann. de Chim. vol. LXXIV, p. 313. This cement was sent by Mr. Belloni, Director of the Imperial School of Mosaic, who considered it as one of the best cements the ancients had employed in the fabrication of their mosaics, and of their pavements in compartments.

On

On comparing these two analyses we find, that the cement contains in 100 parts

Quicklime	56·3
Carbonic acid	41
Vegetable or animal matter	2·7
	100·0

In this cement, we see, the lime, if it were employed quick, has resumed from the air, and in the lapse of time, nearly all the carbonic acid necessary for its saturation. The lime nearly saturated.

This is the first time of my observing this fact. As I have never found the lime in mortar, however ancient, saturated with carbonic acid, I am inclined to suppose, that the vegetable or animal matter, that served as a gluten, promoted the absorption of carbonic acid; or rather, that the cement in question was made with carbonate of lime (whiting), and not with quicklime. This singular. How effected.

In the latter case about 97 parts of carbonate of lime, and 3 of oil, glue, or cheese, must have been employed.

In the former the cement would have been composed of about 56 parts of quicklime to 3 of vegetable or animal matter.

It is obvious, that these proportions, which are found at present to form the cement in question, were not followed in its preparation.

If oil were employed, it would have increased in weight in drying; and then less than 0·03 must have been used, which appears to me impossible.

It is more than probable therefore, that the substance employed was analogous to the caseous part of milk, and then it would have diminished in weight by losing the water it contained, which served to reduce to a paste the lime or carbonate of lime. Its probable composition.

From this analysis it appears, that the cement was very simple; and that those we now compose on the same principle would become equally hard in time.

XIV.

XIV.

I EOROLOGICAL JOURNAL.

	Wind	Pressure. Max.	Min	Med.	Temperature. Max	Min	Med.	Evap.	Rain	
8th Mo.										
Aug. 11	N W	30·16	29·86	30·01	61	42	51·5	—		☽
12	N W	30·16	30·10	30·13	64	50	57	—		
13	S W	30·24	30·10	30·17	73	52	62·5	·33	—	
14	N W	30·25	30·09	30·17	66	47	56·5	—		
15	S W	30·25	29·97	30·11	68	51	59·5	—	—	
16	S W	30·14	29·97	30·055	68	[illegible]	62·5	·37	—	
17	N W	30·13	30·03	30·08	70	45	57·5	—		
18	E	30·03	29·76	29·895	72	55	63·5	—	2	
19	Var.	29·72	29·65	29·685	68	54	61	·30	·35	●
20	W	30·05	29·72	29·885	64	57	60·5	—	5	
21	S W	30·08	30·04	30·06	68	56	62	—		
22	W	30·04	29·92	29·98	71	52	61·5	·41	—	
23	S	29·92	29·73	29·825	68	55	61·5	—	4	
24	E	29·73	29·52	29·625	70	55	62·5	—	—	
25	S W	29·70	29·50	29·60	65	48	56·5	·16	·39	
26	S W	29·78	29·70	29·74	67	56	63·5	—		
27	W	30·07	29·74	29·905	68	44	56	—	1	☾
28	Var.	30·11	30·03	30·07	66	51	58·5	·42		
29	S W	30·17	29·96	30·065	69	46	57·5	—	2	
30	N W	30·20	30·13	30·165	69	47	58	·24		
31	S W	30·02	29·97	29·995	71	[illegible]	62	—		
9th Mo.										
Sept. 1	N W	30·22	30·02	30·12	68	45	56·5	—		
2	N	30·29	30·26	30·275	65	45	55	—		○
3	N E	30·29	30·24	30·265	64	53	58·5	·34		
4	N E	30·24	30·18	30·21	62	53	57·5	—		
5	E	30·18	30·15	30·165	71	52	61·5	—		
6	E	30.17	30·13	30·15	73	44	58·5	·35		
7	N E	30·19	30·13	30·16	72	43	57·5	—		
8	E	30·20	30·17	30.185	74	47	60·5	·22		
		30·29	29·50	30·025	74	[illegible]	59·20	3·14	·88	

N. B. The observations in each line of the Table apply to a period of twenty-four hours, beginning at 9 A. M. on the day indicated in the first column. A dash denotes, that the result is included in the next following observation.

NOTES

NOTES.

Eighth Mo. 11. *Cumulostratus*, dense about noon, but which soon after dispersing, a brilliant sunset ensued. 12. a. m. cloudy: wind S.W. 13. A few drops at intervals: rain in the S. by inosculation. 14. a. m. *Cumulus*, with haze gradually increasing above: p. m. clouds below disperse: a fine elevated veil of *cirrus*, coloured at sunset. 15. Elevated clouds, with traces of *cumulus*: some large drops about noon: at sunset, the western sky richly coloured with red and yellow, on *cirrocumulus* passing to *cirrostratus*: windy night. 16. a. m. Windy: p. m. small rain: clear evening, with coloured *cirrus* and *cirrocumulus*. 18. Evening, large *cirri*, pointing upwards. 19. a. m. Thunder showers, chiefly to S. S. W. and N. A strong variable charge in the insulated conductor. 6 p. m. Fair and windy, with *cumulostratus*. 20, 21. Windy: much dew. 22. Light rain a. m.: showers p. m. 23. Misty morning: *cumulus*, with *cirrostratus* from the S.: about *one*, these inosculated, and showers prevailed, p. m. 24. Misty morning: *cumulostratus*: a few drops of rain: evening, *cirrostratus*. 25. Misty, and raining at 8½ a. m. Wind S. E. Evening, *cumulostratus* evaporating, beneath a veil of *cirrus*, which at the moment of sunset, was of a light silver grey, and during twilight, passed through yellow, orange, red, and purple, to dull grey; and lastly became again somewhat red: much dew, with a very moist air. 26. A small lunar halo, on clouds moving in a northerly current. 27. Windy, a. m.: small rain, evening: much dew. 28. Windy. 30. a. m. *Cirrus*, with points dependent and crossing, and *cumulus* forming beneath: at 9 p. m. *Cirrocumulus*, with much dew. The barometer unsteady. 31. Fine day: *cumulus*, *cirrus*, *cirrocumulus*: a diffused blush on the twilight, which begins to be very luminous.

RESULTS.

Wind westerly, with little exception, to the time of full moon, when it came round by N. to the Eastward.

Barometer: highest observation 30·29 in. lowest 29·50 in.
Mean of the period 30·025 in.

Therm.: highest observation 74°, lowest 42°. Mean of the period 59·20°.
Evaporation 3·14 in. Rain 0·88 in.

L. HOWARD.

PLAISTOW,
Ninth Month 26, 1811.

XV.

Remarks on the Inclination of the Stems of Plants toward the Light: *by M.* DECANDOLLE *.

Inclination of stems of plants toward the light, not from volition or instinct, but known laws of vegetation.

OF all the phenomena that living vegetables exhibit, there are few appear so extraordinary, as the energy and constancy with which their stems incline toward the light. Not only has no explanation been given hitherto of this fact by any physiologist, but writers have even been found, who, more of the poet than of the naturalist, have ascribed this tendency to some kind of instinct or volition in plants. I think I can prove in a few words, that it is a simple and necessary consequence of the known laws of vegetation. What I have to say in this respect will even appear of so elementary a nature, that every one will be surprised not to have met with in all books: and that I shall be pardoned for writing it only on account of the wanderings, into which some have gone on the subject.

Etiolation, not a general but topical affection.

Every one knows, that the state of silvery whiteness and extraordinary elongation, acquired by plants that grow in darkness, is designated by the term etiolation. All who have studied this disease know, that it is not a general disease, but a local affection; as I have satisfied myself by direct experiments. If we expose to the light of day an etiolated plant, in two days it will acquire a green colour perceptibly similar to that of plants, which have grown in open daylight. If we expose to the light one part of the plant, be it leaf or branch, this part alone will become green. If we cover any part of a leaf with an opake substance, this place will remain white, while the rest becomes green. The whiteness of the inner leaves of cabbages is a partial etiolation, and a thousand other examples might easily be quoted. Etiolation therefore is certainly a local, and not a general disease.

Various degrees of it.

On the other hand it is equally certain, that between complete etiolation and complete verdure every possible intermediate degree exists, determined by the intensity of the

* Mém. de la Soc. d'Aroueil, vol. II, p. 104.

light.

light. Of this any one may easily satisfy himself, by attending to the colour of a plant exposed to the full daylight; it exhibits in succession all the degrees of verdure. I had already seen the same phenomenon in a particular manner, by exposing etiolated plants to the light of lamps. In these experiments (inserted in vol. I, of the *Mém. des Savans étrangers*) I not only saw the colour come on gradually according to the continuance of the exposure to light; but I satisfied myself, that a certain intensity of permanent light never gives to a plant more than a certain degree of colour. The same fact readily shows itself in nature, when we examine the plants that grow under shelter or in forests, or when we examine in succession the state of the leaves, that form the heads of cabbages.

Etiolated plants exposed to artificial light.

Now let us examine the state of a plant, that is not equally enlightened on all sides, as we see them in forests; and still better in plants cultivated in hothouses, or in common rooms. That part of the stalk which is exposed to the least light must necessarily be a little more etiolated than the other; consequently it must elongate itself a little more, while the fibres on the side next the light must become on the contrary a little more short and stiff. But it is evident, that this inequality of elongation between the fibres of the two opposite sides cannot take place without the extremity of the stalk tending to incline toward the side where the fibres are shortest, that is to say, on the side next the light.

Plants not equally exposed to light in forests, hot houses, or rooms,

Incline to the light from partial etiolation.

Thus it appears, if this theory be true, that the energy, with which plants incline themselves toward the light, must be proportional to the inequality of the light they receive on opposite sides, and to the greater or less propensity to etiolation, that each plant, or part of a plant, possesses, in consequence of its structure. This I shall proceed to prove by facts, most of them, it is true, already known, but which will be so many confirmations of my hypothesis.

This inclination proportional to the degree of the affection.

The parts of plants liable to etiolation alone possess this tendency to incline toward the light. Of this any one may satisfy himself, by examining the branches directed toward the windows in a greenhouse not well lighted. He will find, that they are always the young shoots, capable of emitting oxigen gas, that direct themselves toward the light; and that

Only parts of plants liable to etiolation incline toward the light.

that the energy of this direction is greatest in the most herbaceous stalks, in which the phenomenon of etiolation is also most remarkable. In forests the woody branches or stems themselves may frequently be observed twisted to gain an open place; but this is because the unequal distribution of light has continued several years; the branches were bent in their green state, and have acquired solidity in that in which they are found. Of this I have satisfied myself by direct measures. Permit me here to observe, that it may be possible to avail ourselves of this property of vegetables, to formed curved timber for the purposes of the arts, by directing the light on certain trees in a suitable manner.

The inclination in general proportionate to the flexibility,

In the instances I have quoted it may be supposed, that, if old branches do not bend, it is solely on account of their hardness: and indeed it is evident, that, the more flexible the branch, the more will it be bent by the same quantity of partial etiolation; but a striking example will prove,

but does not take place in plants incapable of decomposing carbonic acid, as dodder.

that the inclination toward the light does not take place in the most flexible branches, when they want the faculty of decomposing carbonic acid gas by means of light. This example is dodder. I have satisfied myself by direct experiments, that it does not incline itself toward the light; that, placed under water in the sun, it does not decompose carbonic acid gas, and consequently can lengthen itself equally on both sides, though unequally illumined.

It depends on a partial elongation of the vessels,

The whole of the phenomenon then consists in the partial elongation produced by etiolation. But it is known, that the elongation takes place chiefly in the vessels, which draw along with them as it were the cellular texture. Consequently, the more vessels there are in a plant, or a part of a plant, the more it ought to incline toward the light. In

and is scarcely perceptible in plants formed of spherical cells.

plants totally destitute of vessels, this inclination must be scarcely perceptible, because the rounded cells grow nearly alike in all directions: hence this inclination toward the light is next to nothing in the cryptogamia, as in certain algæ composed solely of rounded cellular texture. Those of the cryptogamia, which, as the mosses for example, are composed of two sorts of cellular texture, one with rounded the other with tubular cells, approach the vascular plants in consequence of the latter, which is capable of more or less

less elongation; and in these we may observe a slow and feeble inclination toward the light. Lastly, plants furnished with vessels, and of these plants the stems, in which vessels most abound, exhibit this inclination most forcibly.

I conceive therefore I have proved, by this combination of facts, that the hitherto unexplained phenomenon of the inclination of the stems of plants toward the light is readily reducible to the known laws of etiolation.

XVI.

On the Forcing-houses of the Romans, with a List of Fruits cultivated by them, now in our Gardens. By the Right Hon. Sir Joseph Banks, *Bart. K. B. P. R. S. &c.* *

The Romans had forcing-houses.

Mr. A. Knight was the first person among us members of the Horticultural Society, who observed, in reading Martial, strong traces of the Romans having enjoyed the luxury of forcing-houses. I shall cite the principal passages upon which he has founded this observation, the truth of which is not likely to be controverted, and add such remarks as present themselves upon the Roman hot-houses, with a few words on the subject of our own.

The first epigram is as follows:

Proofs of this.

> Pallida ne Cilicum timeant pomaria brumam,
> Mordeat et tenerum fortior aura nemus,
> Hibernis objecta notis specularia puros
> Admittunt soles, et sine fæce diem, &c.
>
> *Martial*, lib. viii, 14.

> Qui Corcyræi vidit pomaria regis,
> Rus, Entelle, tuæ præferat ille domus.
> Invida purpureos urat ne bruma racemos,
> Et gelidum Bacchi munera frigus edat;
> Condita perspicua vivit vindemia gemma,
> Et tegitur felix, nec tamen uva latet.

* Trans. of the Hort. Soc. vol. I, p. 147.

Fœmineum lucet sic per bombycina corpus:
Calculus in nitida sic numeratur aqua.
Quid non ingenio voluit natura licere?
Autumnum sterilis ferre jubetur hiems.
Martial, lib. viii, 68.

The four last lines of the first epigram are omitted, as having no reference whatever to the subject.

Their mode of forcing cucumbers.

From these passages, and from that of Pliny, in which he tells us that Tiberius, who was fond of cucumbers, had them in his garden throughout the year by means of (*specularia*) stoves, where they were grown in boxes, wheeled out in fine weather, and replaced in the nights or in cold weather, *Pliny, book* xix, *sect.* 23, we may safely infer, that forcing-houses were not unknown to the Romans, though they do not appear to have been carried into general use.

Flues in common use among them.

Flues the Romans were well acquainted with; they did not use open fires in their apartments as we do, but, in the colder countries at least, they always had flues under the floors of their apartments. Mr. Lysons found the flues, and the fire-place whence they received heat, in the Roman villa he has described in Gloucestershire; in the baths also, which no good house could be without, flues were used to communicate a large proportion of heat for their sudatories, or sweating apartments.

They used talc instead of glass.

The article with which their windows were glazed, if the term may be used, was *talc*, or what we call Muscovy glass, (*lapis specularis*). At Rome, the apartments of the bettermost classes were furnished with curtains (*vela* *), to keep away the sun; and windows (*specularia* †), to resist cold; so common was the use of this material for windows, that the glazier, or person who fitted the panes, had a name, and was called *specularius*.

The first epigram relates to a peach-house.

On the epigrams the following remarks present themselves. The first in all probability described a peach-house, the word pale, which is meant as a ridicule upon the prac-

Transparent bee-hives.

* Ulpian l. Quæsitum 12. The Romans also made transparent bee-hives of the same material. Pliny, lib. xxi, sect. 47.

† Quamvis cœnationem velis et specularibus muniant. *Seneca*.

tice

tice, gives reason for this supposition; we all know that peaches grown under glass cannot be endowed either with colour or with flavour, unless they are exposed by the removal of the lights, from the time of their taking their second swell, after stoning, to the direct rays of the sun: if this is not done, the best sorts are pale green when ripe, and not better than turnips in point of flavour; but it is not likely, that a Roman hot-house should, in the infancy of the invention, be furnished with movable lights, as ours are. The Romans had peaches in plenty both hard and melting*. The flesh of the hard peaches adhered to the stones as ours do*, and were preferred in point of flavour to the soft ones†.

They had both sorts in plenty.

The second epigram refers most plainly to a grape-house, but it does not seem to have been calculated to force the crop at an earlier period than the natural one; it is more likely to have been contrived for the purpose of securing a late crop, which may have been managed by destroying the first set of bloom, and encouraging the vines to produce a second. The last line of the epigram, which states the office of the house to be that of compelling the winter to produce autumnal fruits, leads much to this opinion.

The second epigram describes a grape house for late crops.

Hot-houses seem to have been little used in England, if at all, in the beginning of the last century. Lady Mary Wortley Montagu, on her journey to Constantinople, in the year 1716, remarks the circumstance of pine-apples being served up in the desert, at the Electoral table at Hanover, as a thing she had never before seen or heard of; *see her Letters.* Had pines been then grown in England, her ladyship, who moved in the highest circles, could not have been ignorant of the fact. The public have still much to learn on the subject of hot-houses, of course the Horticultural Society have much to teach,

Hot-houses scarcely known a century in England.

They have hitherto been too frequently misapplied under the name of forcing-houses, to the vain and ostentatious purpose of hurrying fruits to maturity, at a season of the year, when the sun has not the power of endowing them with their natural flavour; we have begun however to apply

Misapplied as forcing-houses.

* Pliny, lib. xv, sect. 34. † Pliny, lib. xv, sect. 11.

them

Their proper uses.

them to their proper use, we have peach-houses built for purpose of presenting that excellent fruit to the sun, w his genial influence is the most active. We have others the purpose of ripening grapes, in which they are secu from the chilling effects of our uncertain autumns, and have brought them to as high a degree of perfec here, as either Spain, France, or Italy can boast of. have pine-houses also, in which that delicate fruit is ra in a better style than is generally practised in its native tertropical countries; except, perhaps, in the well mana gardens of rich individuals, who may, if due care and att tion is used by their gardeners, have pines as good, but c not have them better, than those we know how to grow England.

They will be much improved.

The next generation will no doubt erect hot-houses much larger dimensions than those, to which we have therto confined ourselves, such as are capable of rais trees of considerable size; they will also, instead of he ing them with flues, such as we use, and which waste in walls that conceal them more than half of the warmth th receive from the fires that heat them, use naked tubes metal filled with steam * instead of smoke. Garden will then be enabled to admit a proper proportion of to the trees in the season of flowering, and as we alrea are aware of the use of bees in our cherry-houses to dist bute the pollen, where wind cannot be admitted to dispe it, and of shaking the trees when in full bloom, to put t pollen in motion, they will find no difficulty in setting t shyest kinds of fruits.

Fruits that will soon be cultivated in them.

It does not require the gift of prophecy to foretell, th ere long the aki and the avocado pear of the West Indi the flat peach, the mandarine orange, and the litchi China, the mango†, the mangostan, and the durion of t East Indies, and possibly other valuable fruits, will be fr

* A neat and ingenious fancy for heating melon frames by steam a peared in the Gentleman's Magazine for January, 1755.

† The mango was ripened by Mr. Aiton, his Majesty's gardener, the Royal Gardens at Kew, in the autumn of 1808, who has frequent ripened fruits of the mespilus japonica, which is a good but not a s perior fruit.

que

quent at the tables of opulent persons; and some of them, perhaps in less than half a century, be offered for sale on every market day at Covent Garden.

Subjoined is a list of those fruits cultivated at Rome, in the time of Pliny, that are now grown in our English gardens.

Modern fruits cultivated by the Romans.

Almonds.—Both sweet and bitter were abundant.

Apples.—22 sorts at least: sweet apples (*melimala*) for eating, and others for cookery. They had one sort without kernels.

Apricots.—Pliny says of the apricot (*armeniacu*) quæ sola et odore commendantur, *lib.* xv, *sect.* 11. He arranges them among his plums. Martial valued them little, as appears by his epigram, xiii, 46.

Cherries were introduced into Rome in the year of the city 680, 73 A. C. and were carried thence to Britain 120 years after, A. D. 480. The Romans had eight kinds, a red one, a black one, a kind so tender as scarce to bear any carriage, a hard fleshed one (*duracina*) like our bigarreau, a small one with a bitterish flavour (*laurea*) like our little wild black, also a dwarf one not exceeding three feet high.

Chestnuts.—They had six sorts, some more easily separated from the skin than others, and one with a red skin; they roasted them as we do.

Figs.—They had many sorts, black and white, large and small, one as large as a pear, another no larger than an olive.

Medlars.—They had two kinds, the one larger, and the other smaller.

Mulberries.—They had two kinds of the black sort, a larger and a smaller. Pliny speaks also of a mulberry growing on a brier: *Nascuntur et in rubis*, *l.* xv, *sect.* 27, but whether this means the raspberry, or the common blackberry does not appear.

Nuts.—They had hazle-nuts and filberds; (has quoque mollis protegit barba) *l.* 15, *sect.* 24: they roasted these nuts.

Pears.—Of these they had many sorts, both summer and winter fruit, melting and hard, they had more than thirty-six

six kinds, some were called libralia: we have our pound pear.

Plums.—They had a multiplicity of sorts, (ingens turba prunorum) black, white, and variegated, one sort was called asinina, from its cheapness, another damascena, this had much stone and little flesh: from *Martial's Epigram*, xiii, 29, we may conclude, that it was what we now call prunes.

Quinces.—They had three sorts, one was called chrysomela from its yellow flesh; they boiled them with honey, as we make marmalade. See *Martial*, xiii. 24.

Services.—They had the apple shaped, the pear-shaped, and a small kind, probably the same as we gather wild, possibly the azarole.

Strawberries—they had, but do not appear to have prized, the climate is too warm to produce this fruit in perfection unless in the hills.

Vines.—They had a multiplicity of these, both thick skinned (duracina) and thin skinned: one vine growing at Rome produced 12 amphoræ of juice, 84 gallons. They had round berried, and long berried sorts, one so long, that it was called dactylides, the grapes being like the fingers on the hand. Martial speaks favourably of the hard skinned grape for eating, xiii, 22.

Walnuts.—They had soft shelled, and hard shelled, as we have: in the golden age, when men lived upon acorns, the gods lived upon walnuts, hence the name juglans, Jovis glans.

Fruit cultivated in England in the 15th century.

As a matter of curiosity, it has also been deemed expedient, to add a list of the fruits cultivated in our English gardens, in the year 1573: it is taken from a book entitled Five Hundred Points of good Husbandry, &c., by Thomas Tusser.

Thomas Tusser.

Thomas Tusser, who had received a liberal education at Eton school, and at Trinity Hall, Cambridge, lived many years as a farmer in Suffolk and Norfolk: he afterward removed to London, where he published the first edition of his work under the title of One Hundred Points of good Husbandry, in 1557.

In his fourth edition, from which this list is taken, he first

first introduced the subject of gardening, and has given us not only a list of the fruits, but also of all the plants then cultivated in our gardens, either for pleasure or profit, under the following heads.

Objects of gardening at that time.

Seedes and herbes for the kychen, herbes and rootes for sallets and sawce, herbes and rootes to boyle or to butter, strewing herbes of all sorts, herbes, branches, and flowers for windowes and pots, herbs to still in summer, necessarie herbes to grow in the gardens for physick not reherst before.

This list consists of more than 150 species, beside the following fruits.

List of old English fruits.

Apple trees of all sorts	Mulberry
Apricockes	‡Peaches white and red
Barberries	Peeres of all sorts
Boollesse black and white	Peer plums black and yellow
Cherries red and black	Quince Trees
Chestnuts	Raspis
*Cornet Plums	§ Reisons
Damisens white and black	Small Nuts
Filberds red and white	Strawberries, red and white
Goseberries	Service Trees
Grapes white and red	Wardens white and red
Grene or Grass plums	Wallnuts
† Hurtil-berries	Wheat Plums
Medlers or Merles	

* Probably the fruit of cornus mascula, commonly called cornelian cherry.

† *Hurtleberries*, the fruit of *vacinium vitis idea*, though no longer cultivated in our gardens, are still esteemed and served up at the tables of opulent people in the counties that produce them naturally. They are every year brought to London from the rocky country, near Leith Tower in Surry, where they meet with so ready a sale among the middle classes of the people, that the richer classes scarcely know that they are to be bought.

‡ The *yellow fleshed peach* now uncommon in our gardens, but which was frequent 40 years ago, under the name of the orange peach, was called by our ancestors melicoton.

§ By *reisons* it is probable that currants are meant; the imported fruit of that name of which we make puddings and pies was called by our ancestors *raisin de Corance*.

Though

Though the fig is omitted by Tusser, it was certainly introduced into our gardens before he wrote. Cardinal Pole is said to have imported from Italy that tree, which is still growing in the garden of the archbishop's palace, at Lambeth.

XVII.

Method of preparing Ox-Gall in a concentrated state for Painters, and for other Uses: *by* RICHARD CATHERY, *No.* 14, *Mead's-row, Lambeth**.

SIR,

A method of keeping gall desirable.

IT has been long a desideratum to find out a method of preparing ox-gall for the use of painters, so as to avoid the disagreeable smell, which it contracts by keeping in a liquid state, and at the same time to preserve its useful properties. I have invented a method of doing it with very little expense, which will be a great saving to those who use gall, as it will prevent it from putrifying, or breeding maggots.

Its uses to artists,

One gall prepared in my method will serve an artist a long time, as it will keep a great number of years. It will be a convenient article for use, as a small cup of it may be placed in the same box which contains other colours, where it will be always ready. The qualities of gall are well known to artists in water-colours, particularly to those who colour prints, as many colours will not, without gall, work free on such paper, on account of the oil that is used in the printing-ink.

The artists who make drawings in water-colours also use gall in the water which they mix their colour with, as it clears away that greasiness, which arises from moist hands upon paper, and makes the colour work clear and bright. My preparation is ready for use in a few minutes, all that is necessary being to dissolve about the size of a pea of it in a table-spoonful of water.

* Trans. of Soc. of Arts, vol. XXVIII, p. 106. Ten guineas were voted to Mr. Cathery for this invention.

It

It is also of great use to housekeepers, sailors, and others, to clean woollen clothes from grease, tar, &c.; and will be found advantageous for many other purposes. and for cleaning cloth.

If it should meet with the approbation of the Society, I have no objection to prepare it for sale.

I am, Sir,
Your obedient Servant,
RICHARD CATHERY,
Botanical-Colourer.

Process for preparing Ox-Gall in a concentrated state, by by Mr. Cathery.

Take a gall fresh from the ox, and put it into a basin, let it stand all night to settle, then pour it off from the sediment into a clean earthen mug, and set it in a saucepan of boiling water over the fire, taking care that none of the water gets into the mug. Let it boil till it is quite thick, then take it out and spread it on a plate or dish, and set it before the fire to evaporate; and when as dry as you can get it, put it into small pots, and tie papers over their tops to keep the dust from it, and it will be good for years*. Method of preparing it.

Certificates were received from Mr. Gabriel Bayfield, No. 9, Park Place, Walworth; and Mr. William Edwards, No. 9, Poplar Row; both botanical colourers; stating, that they have used the ox-gall prepared by Mr. Cathery, and find it to answer better than gall in a liquid state; that this preparation is free from disagreeable smell, and is much cheaper, as one ox-gall thus prepared will last one person for two years, and be as fresh as if just taken from the ox. Testimonies of its usefulness to artists.

A Certificate was received from Mr. James Stewart, No. 26, St. Martin's Street, Leicester Square, stating, that he lately belonged to his Majesty's ship the Vestal frigate, and that he took out with him, in a voyage to Newfoundland, a large pot of the prepared ox-gall, for the purpose of washing his greasy clothes for two years; that he found it very serviceable, and to keep its virtue as well as the first day. And at sea.

* Gall will keep some time, if merely boiled so as to separate the albuminous part, agreeably to the directions of Mr. J. Clark, or professor Proust. See Journal, vol. XVII, p. 341. C.

XVIII.

XVIII.

from Mr. VITALIS, *Professor of Chemistry at Rouen, [...]r. Bouillon-Lagrange, on the Amalgam of Mercury and Silver called Arbor Dianæ**.

Arbor Dianæ capable of being taken out of the vessel in which it is formed.

THE process mentioned by Baumé, which is generally followed for obtaining the peculiar amalgam of mercury and silver, known in chemistry by the name of arbor Dianæ, is not the only one capable of affording those beautiful crystalline figures, that distinguish this curious production. I have obtained the same object by an alteration in the common method, that enables me very easily to remove the metallic arborization from the liquid in which it is formed, and thus to keep it in another vessel unaltered.

Process.

The process is very simple. In the nitric solutions of mercury and silver, both fully saturated, and diluted with the quantity of water directed by Baumé, I suspend 5 or 6 drachms of very pure mercury, tied up in a piece of fine linen doubled. The metallic solutions soon penetrate to the mercury enclosed in the cloth; and we presently perceive clusters of beautiful needles forming round it, and adhering to the nucleus of mercury. These needles gradually increase in bulk, and in a short time extend above an inch in length.

Method of removing it.

When the metallic arborization ceases to increase, the bag loaded with beautiful needly prisms, which appear to me to be tetraedral, is to be taken out; and, by means of the silk thread, with which it was tied up, fastened to a cork. The whole is then to be suspended under a small glass jar, in the midst of which the metallic crystals may be preserved as long as we please. I have a crystallization of this kind in my laboratory, which has retained all its beauty these two years.

Probably the proportions of the amalgam different,

The solidity of the metallic crystals obtained by my method, compared with the weakness of the threads that form the common arbor Dianæ, lead me to suppose, that the proportions of mercury and silver are not the same in the two cases; and I would have endeavoured to ascertain the difference, if Mr. Vauquelin, to whom I have communicated the fact, had not undertaken, to remove every doubt on this head by a comparative analysis.

* Annales de Chim. Vol. LXII, p. 93.

The

The different configurations of the crystals too may give rise to some interesting researches, which I have not yet had time to pursue. as are the shapes of the crystals.

SCIENTIFIC NEWS.

MR. Heinekin having exposed a solution of very pure carbonate of potash to the action of the galvanic pile, found, that in three or four days the liquid next the negative pole had acquired a golden yellow colour; and a very decided smell of oximuriatic acid was perceptible. With the nitrates of silver and of mercury the yellow liquid formed a grumous precipitate; and it completely destroyed the colour of litmus blue, and of ink. The liquid next the positive pole was highly caustic. The conclusions he draws are, that potash and oximuriatic acid are composed of the same principles, or of carbon, hidrogen, and oxigen in different proportions. Solution of carbonate of potash decomposed by galvanism, and oximuriatic acid formed?

It is a circumstance not a little remarkable, that Mr. Curaudau and Dr. Davy were led to form similar notions of the oximuriatic acid about the same time at Paris and London. From the circumstances of the times it may be presumed, that there could be no communication between them; but it is probable, that, though the merit of discovery is equally due to both these gentlemen, if it be not a fallacy as some suppose, the priority rests with Mr. Curaudau, as his paper was read to the French Institute on the 5th of March, 1810. Opinion of Mr Curaudau on the simple nature of oximuriatic gas.

The following is one of the experiments, on which Mr. Curaudau founds his opinion. By combining oximuriatic gas directly with nitrate of silver a precipitate is formed, without any oxigen being disengaged; and, as the weight of the precipitate thrown down is proportional to that of the gas employed, he infers, that it is a compound of the muriatic radical and silver. He infers farther, that in this process the hidrogen of the acid disoxidates the silver; and the silver thus disoxidated enters directly into combination with the muriatic radical, so as to form a *muriuret* of silver. Hence we see, why potash in the humid way, and carbon in the Oximuriatic gas forms a union with metallic silver,

the dry, will not decompose this salt: and why, on the other hand, hidrogen so easily effects the reduction of the metal. The proportions assigned by Mr. Curaudau to the muriatic acid are one part of hidrogen to thirty-three of oximuriatic gas.

and with 0·03 of hidrogen composes muriatic acid.

Three meteoric stones.

On the 23d of November, 1810, at half after one in the afternoon, three atmospheric stones fell in the commune of Charsouville, canton of Meung, department of the Loiret. Their fall was accompanied by a series of detonations, which preceded it, and lasted some minutes. The sound of the explosions, to the number of three or four, followed by a rumbling noise occasioned by the echoes, was heard as loud at Orleans as at the place where the stones fell. It is said it was equally loud at Montargis, Salbri, Vierzon, and Blois, at all which places it excited alarm, being supposed to arise from the blowing up of a powder magazine. The explosions must therefore have taken place at a great height.

Circumstances of their fall.

The fall of these stones was perpendicular; and without the appearance of any light, or ball of fire. One fell at Montelle but has not been found. The other two fell one at Villenai, the other at Moulin Brûlé. All these places are within the distance of a mile. One of the stones weighed about twenty pounds; it made a hole in the ground just large enough for its admission, in a perpendicular direction, driving up the earth to the height of eight or ten feet. The stone was taken out half an hour afterward, when it was still so hot, that it could scarcely be held in the hands. It had a strong smell of gunpowder, which it retained till it was quite cold. The second stone formed a similar hole three feet deep. It weighed forty pounds, and was not taken out of the ground for eighteen hours after its fall, when it was without heat.

The stones described.

These stones were both shapeless masses, irregularly rounded at all their angles. They contain rather more ferruginous globules, than those that fell at l'Aigle, in Normandy; these globules are somewhat larger; and the colour of the stone, when broken, is lighter. They are quickly oxided, very heavy, sufficiently hard to scratch glass, broken with difficulty, and the fracture is irregular and

and very fine grained. The external crust is a quarter of a line thick, and of a blackish gray colour. The substance of the stone is marked with a few black lines, irregular, very distinct, and from half a line to two lines broad. They traverse it indiscriminately in all directions, like the veins of certain marbles. Does not this seem to indicate, that they existed previous to their fall, and were formed in the same manner as rocks, and not in the atmosphere? The day when these stones fell was remarkably calm and serene; the sun shone as bright as in one of the finest days of autumn; and not a cloud appeared above the horizon.

Directions for sailing to the East Indies &c.

Directions for sailing to and from the East Indies, China, New Holland, Cape of Good Hope, and the interjacent parts, compiled chiefly from Original Journals at the East India House, and from Journals and Observations made during Twenty-one Years Experience navigating in those Seas; by James Horsburg, F. R. S. Part I, published 1809, quarto, 389 full pages with side notes, contents, and a copious index.—Part II, corresponding size and type, 506 pages, just published. Sold by Black, Parry, and Kingsbury.

This valuable publication cannot fail to be of great utility to British navigators, who trade to the southward of the equator, as well as those belonging to his Majesty's navy. Exclusive of sailing directions and local descriptions of winds, weather, currents, ports, headlands, islands, coasts, dangers, &c., the geographical situations of all the particular headlands, islands, ports, and dangers, are stated from actual observations of sun, moon, and stars, or by good chronometers. The necessity of a work of this nature has long been known to navigators; as, former directories having been compiled from a mass of heterogeneous and *very incorrect* materials, obtained when ships were navigated by dead reckoning, prior to the application of marine chronometers and lunar observations to nautical science; and these directories, for the greater part, having been generally transcribed from each other for nearly a century up to the present time: they are constantly fraught with errour, and of little use in the present improved state of navigation.

Upon

Upon this work the author has bestowed nearly five years of almost constant labour, in order to render it as correct as possible, conformably to the important end he had in view, which was the security of the lives and property of numbers in a great commercial nation. How far this end has been attained, scientific and naval men can justly appreciate.

Medical and Chemical Lectures, St. George's Hospital, and George Street, Hanover Square.

Medical and chemical lectures.

These Medical Lectures will recommence as usual in the first week of October, at eight in the morning; and the Chemical at a quarter after nine, at No. 9, George-street, Hanover-square.

Clinical Lectures are given on the cases of patients registered in St. George's Hospital, every Saturday morning at nine o'clock; by George Pearson, M.D. F.R.S., sen. physician to St. George's Hospital; of the College of Physicians; honorary Fellow of the Imperial Medico-chirurgical Academy of St. Petersburgh, &c.

Lectures on Surgery, and on Physiology.

Lectures on surgery and physiology.

Mr. A. Carlisle F. R. S. F. L. S. professor of Anatomy in the Royal Academy, and surgeon to the Westminster Hospital, will begin his Course of Lectures on the Art and Practice of Surgery, on Tuesday, October 8, at eight o'clock in the evening, at his house in Soho-square.

The subject will be continued on Tuesdays, Thursdays, and Saturdays, at the same hour.

The Diseases and Accidents allotted to the province of Surgery will be fully treated of, and illustrated by Cases from the Lecturer's experience. The different Operations will be demonstrated, and the Anatomy of the Parts explained.

These Lectures combine Views of the Natural History, Physiology, and Pathology of the Human Body, calculated to illustrate the several Processes of Healing, and to afford a compendious View of the Animal Economy.

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

NOVEMBER, 1811.

ARTICLE I.

On a Property of the repulsive Forces, that act on Light: by Mr. MALUS*.

IN my last paper† I announced, that light reflected from the surface of transparent bodies acquires new properties, which distinguish it essentially from that which emanates directly from luminous bodies. New properties of reflected light.

I have since continued my researches on the same subject; and, subjecting the results of my experiments to calculation, I have arrived at some remarkable consequences, which tend to elucidate the mode of action of substances on light. Investigation continued.

I had observed, that, when the light is reflected under a certain angle by the surface of a diaphanous body, it acquires the properties of the rays, that have been subjected to double refraction: and, setting out with this remark, I contrived to modify the rays of light by simple transparent substances, so that they entirely escaped the partial reflection, which is commonly observed at the surface of these substances. I cause any number of these substances to be Reflected light acquires the properties of light doubly refracted. The partial reflection from transparent substances prevented:

* Mém de la Soc. d'Arcueil, vol. II, p. 254. † Journal, p. 95.

 traversed

traversed by a solar ray, without any of its particles being reflected; which furnishes means of measuring with accuracy the quantity of light, that these substances absorb; a problem, which the partial reflection had rendered impossible to be solved.

and from opake polished bodies.

The light that has undergone this modification comports itself in a similar manner with opake polished bodies. Under determinate angles it ceases to be reflected, and is totally absorbed, while within and beyond these angles it is in part reflected from the surface of these bodies.

Direct ray on an unsilvered glass.

When a solar ray is made to fall on a polished glass, that is not silvered, this ray is in part reflected at the first and second surface, and its intensity increases with the angle of incidence, reckoning from the perpendicular: in other words, it is so much the greater, in proportion as the ray is more inclined to the reflecting surface.

Light previously reflected follows a different law.

But if the direct light be subject to this law of intensity, that which has been already reflected follows a very different law, when it is reflected anew by a second glass. In certain directions, instead of increasing in intensity with the angle of incidence, on the contrary it diminishes; and, after having attained a certain minimum, begins to increase according to the same law as the direct light. These minima are relative either to the inclination of the ray to the reflecting surfaces, or to the angles which these surfaces form with each other, so that the light reflected by the second glass is a function of these three angles. This function has an absolute minimum; that is to say, a point at which the intensity of the light reflected by the second glass is altogether null. Calculation has led me directly to the circumstances, that produce this minimum; and I have verified it by a very simple experiment, which I shall proceed to describe.

Angle at which all the light reflected by one glass is absorbed by a second.

If we take two glasses inclined to each other at an angle of 70° 22′: if we then conceive between these two glasses a line making with each an angle of 35° 25′, every ray reflected by one of the glasses parallel to this line will not be reflected anew by the second; it will penetrate it, without any of its particles experiencing the action of the repulsive forces, that produce the partial reflection. Within or beyond

yond the angles I have mentioned, the phenomenon will cease to take place; and the farther we go from these limits, on either side, the greater will be the quantity of light reflected.

This faculty of entirely penetrating transparent bodies, which the light has acquired by its first reflection, it will lose or retain in various circumstances, which I have studied; and thus I have been led to the following law, according to which this singular phenomenon is effected.

This property retained or left under different circumstances.

If a second glass be made to turn round the first reflected ray, *a*, constantly making with it an angle of 35° 25′; and if in a plane perpendicular to this ray we conceive two lines, one, *b*, parallel to the first glass, and the other, *c*, parallel to the second; the quantity of light reflected by the second glass will be proportional to the square of the cosine of the angle included between the lines *b c*: it is at its maximum when these lines are parallel, and null when they are perpendicular. So that the limits of the phenomenon are relative to three rectangular axes, *a*, *b*, *c*, one of which is parallel to the direction of the ray, another to the first reflecting surface, and the third is perpendicular to the two former.

Law of this phenomenon.

For the second glass let us substitute a metallic mirror, and call the rectangular axes of the second ray, analogous to the axes *a*, *b*, *c* of the first, *a′*, *b′*, *c′*. If this ray be received on a polished but unsilvered glass, which makes with it an angle of 35° 25′, we shall perceive the following phenomena, which are independant of the angle of incidence on the metallic mirror. If *b′* be parallel to *b*, that is, if the metallic mirror be parallel to the axis *b*, the ray it reflects will retain its properties with respect to a glass situate parallel to the axis *c′*; it will penetrate it entirely. If *b′* be parallel to *c*, the reflected ray will retain its properties for a glass parallel to the axis *b′*.

Metallic mirror substituted for the second glass.

In the intermediate positions, the quantity of light, that will have retained its property for a glass parallel to the axis *b′*, is proportional to the square of the sine of the angle comprised between the axes *b′ b*; and that which has retained its property with respect to a glass parallel to the

M 2 axis

axis c' is proportional to the square of the cosine of the same angle.

When the metallic mirror makes equal angles with the axes b and c, b' makes an angle of 45° with each; and then the light comports itself in the same manner on a glass parallel to the axis b', or to the axis c'; it seems, in this case, to have resumed all the characters of direct light.

Ray from the metallic mirror dissected by calcareous spar.

If the ray reflected by the metallic mirror be dissected by means of a crystal of calcareous spar, in disposing its principal section parallel to the plane of reflection, the proportion of the intensities of the ray refracted extraordinarily and the ordinary ray is equal to the square of the tangent of the angle included between the two axes, b, b'.

Light reflected several times from metallic mirrors.

If the light be made to undergo several reflections from metallic mirrors, before subjecting it to the action of a second transparent body, the phenomena are analogous to those I have mentioned. If the axis b' of the second ray be parallel to the axis b or c of the first; if the axis b'' of the third be parallel to the axis b' or c' of the second; and so for the rest; the property of the light already laid down will be in no respect altered: but if these axes be inclined to one another, it will be divided with respect to the two consecutive mirrors, according to the law I have mentioned.

Reflected light received on black marble.

If the surface of a polished opake substance, as black marble, be made to turn round the axis c of the first reflected ray, the reflected light will be seen to diminish to a certain point, at which it is null, and beyond which it begins to increase.

Ordinary phenomena of optics explicable on the hypothesis of Huyghens, or of Newton:

All the ordinary phenomena of optics may be explained either on the hypothesis of Huyghens, who supposed them to be produced by the vibrations of an ethereal fluid; or agreeably to the opinion of Newton, who suppcsed them to be produced by the action of bodies on luminous particles, considered as themselves belonging to a substance obeying the attractive and repulsive powers, that serve to explain other physical phenomena. The laws respecting the course of rays in double refraction too may be explained on either hypothesis. But the observations I have related prove, that the phenomena of reflection are different at the same

but those here mentioned not reconcilable

same angle of incidence, which cannot take place on the hypothesis of Huyghens: for we must necessarily conclude from them, not only that light is a substance obedient to the forces that set other substances in action, but also that the form and arrangement of its particles have great influence on the phenomena.

with that of Huyghens.

If we transfer to the luminous particles the three rectangular axes, *a*, *b*, *c*, to which the phenomena I have described are referrible; and if we suppose, that, the axis *a* being still in the direction of the ray, the axis *b* or *c*, from the influence of the *repulsive powers*, becomes perpendicular to the direction of these powers; then all the phenomena of total reflection, and of partial reflection, and the most extraordinary circumstances of double refraction, become consequences of one another, and are deducible from this single law, namely, that;

All the phenomena deducible from a single law.

If we consider, in the transference of the luminous particles, their motion round their three principal axes, *a*, *b*, *c*; the quantity of particles, the axis *b* or *c* of which becomes perpendicular to the direction of the repulsive forces, will always be proportional to the square of the cosine of the angle, which these lines will have to describe round the axis *a*, to take this direction; and reciprocally, the quanquantity of the particles, the axis *b* or *c* of which will approach the nearest possible to the direction of the repulsive forces, will be proportional to the square of the sine of the angle, which these lines will have to describe in their rotation round the axis *a*, to arrive at the plane, that passes through this axis and the direction of the forces.

The law.

In the case of double refraction, and when we consider the phenomena, that are exhibited by two contiguous crystals, we may express this law in the following manner.

If we conceive a plane passing through the ordinary ray and the axis of the first crystal, and a second plane passing through the extraordinary ray and the axis of the second crystal, the quantity of light proceeding from the ordinary refraction of the first, and refracted ordinarily by the second, is proportional to the square of the cosine of the angle comprised between the two planes abovementioned; and the quantity

Law in the case of double refraction.

quantity of light refracted extraordinarily is proportional to the square of the sine of the same angle. If it be the extraordinary ray of the first crystal on which we operate, we obtain a similar result, changing the word ordinary for extraordinary, and reciprocally.

Reflection. With regard to reflection, if we consider, for example, a ray reflected by one glass, with which it makes an angle of 35° 25′, and falling on a second glass at the same angle, the angle comprised between the two surfaces being in other respects arbitrary: we must conceive a plane perpendicular to the first glass, and another perpendicular to the second, passing through this reflected ray; and the quantity of light reflected by the second glass will be proportional to the square of the cosine of the angle comprised between these two planes.

I shall confine myself to a few examples of the application of this law.

Examples of the application of this law. Example 1. When a ray is reflected by the surface of a glass at an angle of 54° 35′, we find, that all its particles are disposed in the same manner; since, if we present perpendicularly to this ray a prism of crystallized calcareous spar, the axis of which is in the plane of reflection, all its particles will be refracted in a single ordinary ray, none being refracted extraordinarily. In this case the analogous axes of these particles are all parallel, since they all comport themselves in the same manner. Let us call the axis of these particles, which are perpendicular to the plane of reflection, *b*. All the particles, of which the axis *c* was perpendicular to that plane, have penetrated the transparent body. If therefore we present to the particles reflected, and under the same angle, a second glass parallel to their axis *c*, they will be found similarly circumstanced with those, which could not be reflected by the first; the ray therefore will penetrate this second glass entirely. In fact, experiment shows, that, under these circumstances, all the particles escape the forces of reflection.

Example 2. When we place two rhomboids of calcareous spar on one another, so that their principal sections are parallel, a solar ray parallel to these principal sections produces but two emergent

emergent rays; those which arise from the ordinary and extraordinary refraction of the first crystal being refracted each in a single ordinary or extraordinary ray by the second. In fact in this case it may be conceived, that, whether the axes of the crystals be parallel, or placed in opposite directions, every ray issuing from the first crystal parallel to its principal section is not divided by the second, for its movement takes place round the axis *b* or the axis *c*; and we have seen by the phenomena of reflection, that, whenever the movement takes place round these axes, the ray is not altered; all the particles preserving the parallelism of their similar axes. The rotation round the axis *a* being the only one, that changes the respective positions of the axes of the particles of a given ray.

When the incident ray makes any angle whatever with the principal sections, the rays that proceed from the double refraction of the first crystal are divided into two by the second, so that we then obtain four emergent rays. In this circumstance however there are two different cases, in which the phenomena are very distinct: that in which the axes of the crystals are parallel, and that in which they are in opposite directions. When the axes are parallel, a very vivid light must be employed, and the plane of incidence must be removed to a sensible distance from that of the principal sections, to be able to perceive the rays refracted ordinarily by one crystal and extraordinarily by the other. In fact, agreeably to the theory, the maximum of intensity of these two rays is not the thirtieth part of that of the ray, which proceeds from the ordinary refraction of the two crystals; which has led those who have written on this subject to imagine, that, when the principal sections and the axes are parallel, the light comports itself in the same manner as in the principal section, whatever be the direction of the incident ray: but if we employ a vivid light, under suitable circumstances, observation accords perfectly with the theory. The phenomenon is much more evident, when the axes are in opposite directions. Example 3.

The extraordinary refraction is produced by a repulsive force, the action of which is proportional to the square of the Extraordinary refraction.

the

the sine of the angle included between the axis of the crystal, and the principal axis, a, of the luminous particle. All the particles, of which the axis b is perpendicular to this force, are refracted ordinarily; and all those, of which the axis c is perpendicular to it, are refracted ordinarily. The particles refracted ordinarily, that escape the repulsive force, are in the same case with those, that escape reflection in the first class of facts I have described.

Double reflection.

The phenomena of double reflection at the second surface of transparent crystals are analogous to those of the refraction in two crystals, the principal sections of which are parallel, and their axes in opposite directions; with the addition of this property common to all diaphanous bodies, that, when the reflecting face is parallel to the axis c of the luminous particles, the reflection at a determinate angle is null.

General remarks.

Thus, without the knowledge of this singular property of transparent substances, the most extraordinary part of the phenomena of double refraction would have remained inexplicable.

I shall not enter more largely into the particulars of the application of the theory I have brought forward, but shall content myself with saying, that it refers to one source a number of facts, which seemed to have no analogy to each other, and the want of connexion in which rendered it almost impracticable to measure them.

I do not pretend to point out the cause of this general property of the repulsive powers that act on light; I merely exhibit the means of connecting the phenomena with each other, of ascertaining them before hand by calculation, and of measuring them with accuracy: at the same time in referring the figures of the luminous particles to three rectangular angles, as those of an octaedron would be, I anticipate nothing respecting the real figure of these particles; but I present the result as a consequence of the calculation, to which I have been led by the analysis of the phenomena that I have observed.

II.

II.

Experiments on the Production of Sound in Vapour: by Mr. Biot*.

AN infinite number of experiments have been made on the manner in which sound is produced and transmitted in different mediums. It has been shown, that it is neither formed nor transmitted in a vacuum; and its transmission through solids and liquids has been examined: but no one, I believe, has yet thought of making these experiments in vapour. Such an inquiry however is well calculated to excite our curiosity; for, setting out with the results that experience has made known with respect to the constitution of the vapour that fills a given space, and applying to them the mathematical principles on which the laws of the minute vibrations of elastic fluids are usually founded, it is evident, that no sound should be produced in vapour.

Production and transmission of sound in vapour not yet examined.

No sound should be produced in vapour.

In fact it is shown, by the experiments of De Luc, Saussure, and Dalton, that the quantity of vapour of water, or of any other liquid, that is formed in a vacuum, depends only on the dimensions of that vacuum and the temperature: so that, if this vapour have an elasticity capable of sustaining the manometer at a certain height, and you compress it slowly, so as to oblige it to occupy a smaller space, the elasticity will not be increased by this compression, as that of a permanent gas would be; but a portion of the vapour will return to the liquid state, without any variation of the manometer; and only so much will remain, as is adapted to the new limits, to which the vacuum is reduced. The reverse will happen, if the space be enlarged instead of diminished: a new quantity of vapour will be formed to fill it, but without any change in the elasticity, or in the manometer. These results have been completely established by the learned gentlemen I have mentioned, and we may easily convince ourselves of their accuracy. It is sufficient to introduce into a barometer a small quantity of any liquid; and to measure the height at which the mercury stands,

Properties of vapour.

* Mém. de la Soc. d'Arcueil, vol. II, p. 94. Read to the Institute, October the 12th, 1807.

after

after it is depressed by the elasticity of the vapour formed. If we then raise or lower the external level of the mercury, the interior column will rise or fall exactly as much in the tube; and thus, according as the space remaining at the top of the tube is diminished or increased, a part of the vapour will be precipitated, or fresh vapour will be formed: but, the temperature remaining the same, the elasticity will not alter.

Vibrations of a sonorous body in it.

Now let us suppose, that a sonorous body begins to vibrate in such a medium; each of its vibrations will diminish the space in one direction, and increase it in the opposite. Thus on one side there will be a small quantity of vapour reduced to the liquid state, and on the other a small quantity of liquid will assume the state of vapour. These condensations and dilatations will take place close to the sonorous body in the small extent of its vibrations, but will not be produced beyond this. Thus the motion will not be propagated through the rest of the fluid mass, and consequently the sound will not be transmitted.

If we suppose these to disengage heat, sound should be produced in it.

Let us next suppose, that the sonorous body, in compressing the vapour by its rapid vibrations, disengages from it mechanically a certain quantity of heat. This supposition is by no means improbable, for we know, that vapour gives out a great deal of heat in its condensation. The vapour of water, for example, according to the experiments of Watt, in returning to the liquid state gives out a quantity of heat, that is capable of raising the temperature of the water thus produced to 525° [977° F.]. If we take this circumstance into consideration, the effects of the sonorous body on vapour will no longer be the same: the portions it compresses will preserve their elastic state, notwithstanding the diminution of the space, in consequence of the heat evolved, which instantly increases their elasticity. On the contrary, in the portion dilated the diminution of temperature, preventing a new evaporation, diminishes the elasticity. The phenomena produced near the sonorous body therefore are of the same nature, as if the vapour became a permanent gas. There will be successive and momentary augmentations and diminutions of elasticity, the effect of which will be transmitted step by step throughout the whole

of

of the fluid mass, so as to permit sound to be produced and transmitted in it.

Experiments on the production of sound in vapour therefore are calculated to decide the question, whether heat be really evolved in an aeriform medium by the effect of the vibrations of sonorous bodies, as we see it in general extricated by any rapid compression. Thus we may subject to decisive proof the ingenious idea of Mr. Laplace, by which he has found means of reconciling the mathematical theory of the transmission of sound in air with the results of experience, taking into account the heat evolved: for, if the effect he supposes do not take place, the vibrations of sonorous bodies in vapour should not produce any sound; and, if they do produce sound, it can be only in consequence of the evolution of heat. This may be brought to the test of experiment.

Induced by these motives, I made some experiments on the subject, which completely succeeded. I then repeated them in a more perfect manner, in the philosophical apartments at Arcueil, with my friend Amadeus Berthollet. Mr. Berthollet, and Mr. Laplace were present at these experiments, and themselves verified the facts I am going to relate. Sound was produced in vapour.

We took a glass globe that held 36 litres [near 38 wine quarts]. Its orifice was closed by a well made cock, so that a vacuum might be made in it, which it preserved with great accuracy. To this cock another could be screwed; so that, by pouring a liquid into the space between them, and closing both, this portion of liquid could be afterward introduced into the globe, without admitting any air from without. The sonorous body was a small bell, suspended within the globe by a slender string fastened to the lower cock. Apparatus described.

A vacuum was first made within the apparatus to the greatest nicety, and even so as to exhaust a great part of the hygrometrical water, that might have existed in the globe, which however was very dry. Then, holding the globe by the cock, we set the bell in motion, so as to satisfy ourselves, that the clapper struck very forcibly against the sides: yet, with all the attention we could bestow, even close to the globe itself no sound could be perceived; so that there was no perceptible sound in a vacuum, agreeably to the experiments of Hawksbee, and all other philosophers, Experiment. in a vacuum no sound.

We

In aqueous vapour sound produced

We then introduced into the globe, in the way I have described, a small quantity of water, part of which was converted into vapour. The sound immediately began to be perceptible, though the density of this vapour was extremely small, the temperature being only 19° [66·2° F.].

proportional to its density.

To increase it, an excess of water was admitted into the globe, and it was placed in a stove at the temperature of 46° [114·8° F.]. The sound then became very perceptible: it could be heard without stooping down to the globe, and even out of the stove through the door. Some water still remained in the globe, so there can be no doubt, that the sound was produced and transmitted in the aqueous vapour.

When the globe was taken out of the stove, the temperature quickly fell: a great part of the vapour therefore, which had been raised in consequence of the temperature, was necessarily precipitated; and accordingly the sound appeared very evidently diminished.

In vapour of alcohol sound louder.

Without any alteration in the apparatus, we introduced the same quantity of alcohol, as we had before of water. The specific gravity of this alcohol was 0·823. The vapour from this mixture possessed of course greater density and elasticity than that of water at the same temperature; and accordingly the sound was much more perceptible: it was heard from one extremity to the other of the rooms that form the philosophical apartments at Arcueil. Sound therefore is produced and transmitted in the vapour of alcohol.

Experiment in vapour of ether.

As a last experiment we tried the vapour of ether. This particularly excited our curiosity, on account of its great elastic force and density, which are known to be very considerable; two circumstances, that must contribute to increase the intensity of the sound. We begun with drying the globe, because the moisture would have diminished the tension of the ether; and then allowed the atmospheric air to enter freely, till it was in equilibrio with the external pressure, which was 0·7613 [29·951 inch.]; and, carrying it into a long walk in the garden, we found, that the sound of the bell was sensible to the distance of 145 met. [158·5 yd.]: beyond this it was so faint, that the perception

Distance at which the sound was heard in atmospheric air.

perception of it was not sufficiently certain. The temperature was 17·75° [63·95° F.]. Having measured by this experiment the intensity of the sound produced in atmospheric air, we again made a vacuum in the globe, and introduced into it a sufficient quantity of sulphuric ether, to leave a surplus above what the temperature could convert into vapour. The specific gravity of this ether was 0·759. The elastic force of its vapour, measured by introducing it under a barometer freed from air, was 0·3549 met. [13·963 inches], at the temperature of 17·75° [63·95° F.]. The globe being filled with this vapour, it was carried to the same place as in the preceding experiment; when we found, that the sound was perceptible to the distance of 131·5 met. [143·7 yards]. This conclusively proves in the most convincing manner, that sound is produced and transmitted in vapour, as well as in a permanent gas. But we have proved, that this can take place only from the effect of instantaneous variations of temperature, occasioned by the vibrations. It evidently follows therefore, that this cause really exists; and that, according to the judicious remark of Mr. Laplace, it becomes indispensable for us to pay attention to it in the mathematical theory of the propagation of sound; though we cannot directly verify it by the application of the thermometer, because this instrument can no more be affected by these successive and momentary variations of heat, than the barometer is by the momentary variations of elasticity, that take place in the production of sound, and of which every one notwithstanding acknowledges the existence.

Ether introduced. Elasticity of its vapour.

Distance at which the sound was heard.

This proves the momentary variations of temperature caused by vibrations according to the theory of Laplace.

III.

Experiments to prove, that Fluids pass directly from the Stomach to the Circulation of the Blood, and thence into the Cells of the Spleen, the Gall Bladder, and Urinary Bladder, without going through the Thoracic Duct. By EVERARD HOME, *Esq. F. R. S**.

HAVING on a former occasion laid before the Society some experiments, to prove, that fluids pass directly from the

Fluids pass from the sto-

* Philos. Trans. for 1811, p. 163.

the

mach into the blood, the cardiac portion of the stomach, so as to arrrive at the circulation of the blood without going through the thoracic duct, the only known channel by which liquids can arrive there; the present experiments are brought to confirm that opinion; but in stating them, I wish to correct an errour, I was led into, in believing that the spleen was the channel, by which they are conveyed.

but not through the spleen.

The passage might be found by tying the thoracic duct.

At the time I made my former communications*, I was conscious, that the facts I had ascertained were only sufficient to open a new field of inquiry; but as I might never be able to make a farther progress in an investigation, beset with so many difficulties, I thought it right to put them on record. Since that time I have lost no opportunity of devising new experiments to elucidate this subject; and the circumstance of Mr. Brodie, the assistant of my philosophical as well as professional labours, having tied the thoracic duct in some experiments which will come before the Society, suggested to me the idea, that, if the thoracic duct was tied, and proper experiments made, there could be no difficulty in ascertaining whether there was any other channel between the stomach and the circulation of the blood.

With this view I instituted the following experiment, which was made on the 29th of September, 1810, by Mr. Brodie, assisted by Mr. William Brande and Mr. Gatcombe. I was unavoidably prevented from being present during the time of the experiment.

Exp. 1, on a rabbit.

Exp. 1. A ligature was passed round the thoracic duct of a rabbit, just before it enters at the junction between the left jugular and subclavian veins: an ounce of strong infusion of rhubarb was then injected into the stomach. In three quarters of an hour some urine was voided, in which rhubarb was distinctly detected, by the addition of potash. An hour and a quarter after the injection of the rhubarb the animal was killed: a dram and half of urine was found in the bladder highly tinged with rhubarb, and the usual alteration of colour took place on the addition of potash. The coats of the thoracic duct had given way opposite the middle dorsal vertebra, and nearly an ounce of chyle was found effused into the cavity of the thorax, beside a considerable quantity

* See Journ. vol. XX, p. 374, and XXI, 103.

in

in the cellular membrane of the posterior mediastinum. Above the ruptured part the thoracic duct was entire, much distended with chyle; and on tracing it upwards, the termination of the duct in the vein was found to be completely secured by the ligature. The lacteal and lymphatic vessels had given way in several parts of the abdomen, and chyle and lymph were extravasated underneath the peritoneum.

In this and the following experiments the infusion of rhubarb was employed in preference to the prussiate of potash, in consequence of its having been found in those I formerly made, that one drop of tincture of rhubarb could be detected in half an ounce of serum, and nothing less than a quarter of a grain of prussiate of potash in the same quantity could be made to strike a blue colour when the test was added. Infusion of rhubarb used as the most sensible test.

Exp. 2. The experiment was repeated upon a dog. In this I was assisted by Mr. Brodie, Mr. William Brande, Mr. Clift, and Mr. Gatcombe. After the thoracic duct had been secured, two ounces of strong infusion of rhubarb were injected into the stomach, and in an hour the dog was killed. The urine in the bladder, on the addition of potash, became deeply tinged with rhubarb. The bile in the gall bladder, by a similar test, was found to contain rhubarb. The lacteal vessels in several parts of the mesentery had burst, and chyle was extravasated into the cellular membrane; the thoracic duct had given way in the lower part of the posterior mediastinum, and chyle was extravasated. Above the ruptured part the thoracic duct was much distended with chyle; it was readily traced to the ligature, by which it was completely secured. Exp. 2, on a dog.

These experiments appeared to establish the fact, that the thoracic duct was not the channel through which the infusion of rhubarb was conveyed to the circulation of the blood, and it now became easy to ascertain, whether it passed through the spleen, by extirpating that organ, and repeating the last experiment. The thoracic duct not the passage.

On the 21st of October, 1810, the following experiment was made with the assistance of Mr. Brodie, Mr. Clift, Mr. Gatcombe, and Mr. Money.

Exp. 3.

Exp. 3, on a dog. The thoracic duct tied, and the spleen extirpated.

Exp. 3. The thoracic duct near its termination was secured in a dog, whose spleen had been removed four days before, and three ounces of infusion of rhubarb were injected into the stomach. In an hour and half the dog was killed, and the urine was found strongly impregnated with rhubarb; and on examination, the thoracic duct was found to be completely secured by the ligature. Several of the lacteals had burst, but the duct itself had not given way; it was greatly distended with chyle and lymph.

The spleen not the passage.

By this experiment it was completely ascertained, that the spleen is not the channel through which the infusion of rhubarb is conveyed into the circulation of the blood, as I had been led to believe, and therefore the rhubarb, in my former experiments detected in the spleen, must have been deposited in the same manner as in the urine, and in the bile.

In the next experiment the termination of the thoracic duct on the left side, and the lymphatic trunk of the right side, both secured.

The detection of this errour made me more anxious to avoid being misled respecting the thoracic duct; and therefore, although there was little probability that the infusion of rhubarb could have passed into the lymphatic vessels, which open into the blood vessels of the right side of the neck, I thought it right, before I proceeded farther, to repeat the experiment, securing the termination of the thoracic duct on the left side, and the lymphatic trunk of the right side, where it empties itself into the angle between the jugular and subclavian vein. This was done on the 28th of October, 1810, with the assistance of the same persons as in the last experiment.

Exp. 4, on a dog.

Exp. 4. The thoracic duct of a dog was tied, as in the former experiment; in doing it the duct was wounded, and about a dram of chyle flowed out; the lymphatic trunk of the right side was then secured. After this, three ounces of infusion of rhubarb were injected into the stomach, and in an hour the dog was killed. The urine and the bile were found distinctly impregnated with rhubarb. On opening the thorax, some absorbent vessels, distended with lymph, were seen on the right side of the spine, entering an absorbent gland on the second dorsal vertebra, and the vasa efferentia from the gland were seen uniting with other absorbent vessels, and extending towards the right shoulder, where they

they formed a common trunk with the absorbents from the neck and axilla; this trunk was found included in the ligature. The thoracic duct was moderately distended with a mixture of chyle and lymph; in tracing it upwards, an opening was seen in it immediately below the ligature, through which the contents readily passed out when pressure was made on the duct: above this opening the duct was completely secured by the ligature. Nearly a dram of the fluid contained in the thoracic duct was collected and tested by potash, but there did not appear to be any impregnation of rhubarb.

Exp. 5. The last experiment was repeated on another dog, on the 21st of January, 1811, with the assistance of Mr. Brodie, Mr. W. Brande, Mr. Clift, and Mr. Gatcombe. The dog was killed an hour after the thoracic duct and lymphatic trunk had been secured, and the infusion of rhubarb had been injected into the stomach. Exp. 5, on a dog.

In tying the right lymphatic trunk, a lymphatic vessel from the thorax going to join it was wounded, from which chyle flowed out in considerable quantity during the whole time of the experiment; a short time before the dog was killed some of it was collected, but on testing it with potash no rhubarb was detected in it.

The urine was found impregnated with rhubarb, as was also the bile from the gall bladder; but both in a less degree than in the last experiment. The lacteal vessels and mesenteric glands were much distended with chyle; and on cutting into the glands chyle flowed out in considerable quantity. Some of this was collected and tested with potash, but showed no evidence of rhubarb being contained in it. The thoracic duct was much distended; it was traced to the ligature, and was found to be completely secured.

Lymphatic vessels from the right side of the posterior mediastinum were seen extending towards the ligature, that had been tied on that side; they were nearly empty; and the trunk formed by the junction of these with the lymphatic vessels from the right axilla, and from the right side of the neck, was seen distinctly included in the ligature.

While Mr. Brodie was tracing the thoracic duct, Mr. William Brande was making an infusion of the spleen, and showed Some rhubarb found in the spleen.

size of the part that joins the stem, which at one time of the year is but little larger, and shows the gatherer* but poorly.

There appears a regular gradation of mechanism in this part of all plants, from those which, having the leaves perfectly sessile, are fastened in such a manner to the stem, as to be absolutely incapable of turning, or moving in any manner, to those plants, the leaves of which move with a touch, and the mechanism of which I have before described in the *mimosa sensitiva*†.

Mechanism increasing from the firs to the mimosas.

I have already with indefatigable pains traced this gradation through 130 genera of plants, differing as much as possible, selecting in each a few to illustrate this truth, and in which the mechanism increases gradually from the firs, the leaves of which move not, and have therefore no spiral wire, to the mimosa, which has it knotted and turned over balls.

The first degree of motion in the peduncle is caused by the simple spiral wires in their cases passing into every diminutive vessel in the leaf. The motion is then as simple as the means, and the leaf is merely drawn nearer, or falls farther from the stalk: but when the spiral wire is doubled or crossed, there appears some diversity of motion, by the leaf not only advancing and retiring, but being able to be drawn on one side. The next gradation is shown by the increase of the peduncle next the stalk, and this increase I have ventured to call the gatherer, because it contracts and dilates to favour the spiral wire. When this is found double, that is, adjoining the leaf, as well as the stem, the motion is very greatly increased, since each of them moves through the third of a circle, as I shall presently show. When there appears a ball within the gatherer, the leaf generally proves to be one of those compound leaves, which close as the evening advances. The gradation from this to the mimosa, or those leaves which move with a touch, seems effected by more balls, and by the spiral wire being knotted in a more complicated manner. It would have been curious to show one of each of these specimens, which I have drawn

Most plants have gatherers.

* The name by which I distinguish the increased part of the peduncle.

† See Journal, vol. XXIV, p. 160.

for

for myself; but they would take up too much room, Sir, in your Journal: I shall therefore give only a specimen of what I mean by a gatherer; and a representation of the leaf-stalk dissected.

No person can have examined a tree with attention, without observing the beautiful arrangement of its leaves; the exquisite manner in which they are prevented from obstructing the light, or keeping the air from each other, and the various curious contrivances (especially with large leaves) manifested in raising or depressing them, so as to prevent their throwing too deep a shade on each other, and on those that are beneath them. It is to the gatherers alone they are indebted for this, to the power the two ends of the peduncle have of turning through the third of a circle, that they are able to place themselves in this manner, and arrange their leaves in such beautiful order, so conducive to their benefit and future health. The beautiful arrangement of leaves.

The peduncle may generally be divided into three parts, and, if it has any mechanism to manage, which it is seldom without, it is always found in two of these parts, that which joins the peduncle to the stem, and that which unites the leaf to the peduncle. Pl. V, fig. 1, is a drawing of the peduncle of the liburnum or cytisus. A B are the two gatherers; and C D are the same extremely magnified, and dissected; it is easy to see, that the spiral wire being much contracted may draw these into various figures, according as it is tight or loose within the gatherers, as it is at *e e*, and may turn them three parts of a circle; and thus make the leaf or leaf-stalk measure a very extensive circumference; and by this means accommodating its neighbour, and placing itself in the most eligible situation, not only for its leaf, but for the buds which are trusted to its care, and generally in the axilla of its peduncle. The gatherers at both ends appear, when much contracted, like a screw at the exterior, and sometimes they are so bent as to be doubled, but at another time you will hardly be able to see that they do gather, so various is their figure. I shall now show a specimen of a leaf-stalk, which comes nearer in gradation to the sensitive plant, one of the medicagoes, differing little from the trifoliums, and many of the diadelphian plants. Explanation of the plate. Description of the medicago polymorpha.

Fig.

Fig. 2 is the plant: B B is the upper gatherer, but it has instead of the under one a stipula, which seems by some means (which I have not yet been able to comprehend) to serve instead. All the trifoliums and numbers of the diadelphian plants, have it thus. Fig. 3 shows this part dissected and explained. I have never found the balls z z except in the medicagoes, and not in all of these. There is not any thing more curious than the substance of which the balls are formed. It strongly resembles the matter of the bark without the inner bark vessels, is extremely watery, is the first part that decays, and appears to serve no other purpose, than to fix the string in its place. It is curious, that at *t*, where the knot comes, there is a fastening which passes entirely through the plant. The gatherers *m* and *n* at the side have no balls. There is another kind of a gatherer of a very curious form, which is found in the papilionaceous tribe. It has but one ball; but the same matter, being collected into a thick lump, is folded into creases (see fig. 4, and the dissection fig. 5, *p q*); and have a ball in a semicircular socket; it turns it to one crease, or the other, by means of the spiral wire. Fig. 5 better displays this, being a side view, and showing how it turns to the right or left, by taking the upper or lower crease, which of course turns the leaves nearly a whole circle. Fig. 6 shows the string when drawn tight in the gatherer. This will serve to prove the thorough mistake of those physiologists, who pretend, that the different parts of a plant may be changed for each other, and make a peduncle or leaf take root. Nature does not execute her work in this careless manner, each part has its separate mechanism, than can perform only the part assigned. If a flower bud is concealed in the peduncle, it may by accident grow, since the lower part of the gatherer, which joins the stem, is full of flower buds: but then it is these that grow, and not the leaf-stalk; nor can there be any thing more different, than the peduncle stem.

Formation of the balls.

Form of the everlasting pea.

A strange mistake.

I shall give no farther examples this time, as what I have already said will be, I hope, sufficient to make what I have drawn understood, and to give some idea of the mechanical management of this part of most plants; accounting for the beautiful arrangement of the leaves of trees: and proving,

not

not only that the spiral wire is the cause of motion in plants, but that the management of a plant is wholly mechanical.

I am, Sir,
Your obliged Servant,
AGNES IBBETSON.

I shall in my next give some account of the form of those sessile leaves, which belong to annuals, and those which are of the order pentandria digynia, as there are many curious particulars, which belong to both, and which I have not at present time to detail.

V.

On the Decomposition of Water in two or more separate Vessels. In a Letter from ADAM ANDERSON, *Esq.*

To W. NICHOLSON, Esq.

SIR,

THOUGH the detection of erroneous statements in matters of science is certainly a more humble task than the discovery or generalizément of facts, it must still be regarded as contributing, at least in some degree, to the progress of true knowledge, in so far as erroneous views have a tendency, not only to supersede experimental investigation, but to waste the energies of the mind in attempts to explain a state of things, which has no real existence in nature. I have been led to this remark, by reflecting on the difficulty, which chemists have hitherto experienced, to explain the transmission of the elements of water, during the decomposition of that fluid by galvanism, when a metallic wire forms part of the circuit, and the experiment is performed in separate receivers.

Detections of errours in science important.

Difficulty in explaining the galvanic decomposition of water in separate vessels.

I have ascertained, beyond the possibility of doubt, that the transmission of oxigen and hidrogen in opposite currents through the connecting wire is, contrary to the assertion of Ritter*, entirely fallacious—that the supposition of

Oxigen and hidrogen not transmitted through the wire in opposite currents.

* Journal, 4to edition, vol. IV, p. 512.

of such a transmission must have arisen, either from an inaccurate mode of performing the experiment, or from a hasty and unwarranted generalizement of the repulsions and attractions supposed to be exerted at the opposite poles of the galvanic battery.

Decomposition of water in a single vessel,

Most of your readers are aware, that, when gold wires proceeding from each extremity of a moderately powerful galvanic battery, in a state of action, are introduced under a receiver filled with water, and inverted over a basin containing the same fluid, as at Pl. VI, fig. 1, the wire P being connected with the zinc side, and the wire N with the negative side, a decomposition of the water immediately ensues, oxigen is evolved at *p*, and hidrogen at *n*. The decomposition even goes on, when the wires are inserted in separate receivers, fig. 2; attended with this remarkable circumstance, that oxigen alone is found in one receiver, and hidrogen alone in the other. As we are forced in the present state of our knowledge, to believe, that a decomposition of the water takes place at the extremity of each wire, we must also admit, that the oxigen evolved at *n* is expelled by the negative, and attracted by the positive point, while the hidrogen evolved at *p* is repelled by the positive, and attracted by the negative point; so that, during the decomposition contrary currents of oxigen and hidrogen are proceeding along the dotted line *n a p*. Nay, we must even admit, that the force of these attractions and repulsions is sufficiently powerful, not only to separate the elements of water from a state of combination, but also to overcome the mechanical tendency so ascend through the water, which these elements possess in their gaseous condition.

and in one vessel with separate receivers.

Supposed repulsion and attraction of the oxigen and hidrogen,

sufficient to prevent their ascent through the water.

Similar phenomena said to take place when the water is in separate vessels.

All this may be admitted without much difficulty; but the fact stated by Ritter is by no means so easily explained; and indeed it has never been yet accounted for, without having recourse to the most improbable supposions. This philosopher affirms, that when the receivers *ab*, *cd*, fig. 3, filled with water, and inverted over separate vessels, A B, C D, are connected by a gold wire, *p n*, if the wires P, N, from the opposite extremities of the battery be immersed intot he water contained in the vessels A B, C D, a decomposition

position of the water in the receivers takes place, accompanied by the same result as before, oxigen alone being found in one of the receivers, viz. *a b*, and hidrogen alone in the other, *c d*. Hence he concluded, that as a decomposition of the water must have taken place at each extremity of the connecting wire, the oxigen must have passed through that wire from *n* to *p*, where it was evolved, and the hidrogen in the contrary direction from *p* to *n*.

The gases supposed to flow through the wires.

This explanation, so much at variance with all our notions of the impermeability of dense metallic substances by gaseous bodies, seems to have been reluctantly adopted by the greater number of chemists; while to a few it has appeared so inadmissible, that, rather than embrace it, they have been led to doubt the truth of the opinions commonly received with respect to the compound nature of water. No person, however, appears to have suspected the accuracy of Ritter's statement, or even to have repeated his experiments with any degree of care. The experiments, which I shall now describe, and which, I trust, will be deemed worthy of a place in your Journal, prove, in the most satisfactory manner, that the transmission of the elements of water in opposite currents through the connecting wire is altogether deceptive, and that the opinion of such a transmission taking place is founded on the want of a due attention to all the circumstances of the experiment.

The improbability of this has led to a doubt of the composition of water.

When I first repeated the experiment of Ritter, the result, I confess, appeared very singular; I saw no way of explaining why the oxigen and hidrogen were found separately, without adopting the opinion of Ritter, or denying that water was a compound of these two elementary substances. At length, however by reflecting more maturely on the subject, I began to suspect, that there might be a positive and a negative point in each receiver taken in conjunction with the corresponding cup, over which it was suspended: that the extremity of the wire P, fig. 3, connected with the zinc side of the battery, being positive, and the water acting as a conductor to the galvanic energy, the positive state would be conveyed through the water to the connecting wire *n p*, so that the extremity *p* would also become positive; while, for

The experiment repeated.

The phenomena explained by the positive and negative states taking place in each receiver.

for a similar reason, the opposite extremity n would become negative: that, consequently, as there was a positive and a negative point in the water connected with each receiver, it was obvious, that the decomposition would be effected by mutual attractions and repulsions subsisting between the elements of water, and the two contiguous points of the interrupted circuit, which were thus immersed in the same fluid; in short, that Ritter had been misled by overlooking the decompositions, which, I conceived, took place at the extremities p and n of the wires connected with the battery.

Experiments to prove this.

I accordingly adopted a new arrangement, as at fig. 4. I caused the wires proceeding from the battery to pass through the upper part of the receivers (which were hermetically sealed) and then placed the receivers over the connecting wire $p\,n$, supported on a stand, and passing through the two glass capsules A B. By this disposition of the wires connected with the battery, I was sure of collecting any gasses which might be evolved at their extremities. The result answered my expectation. I now obtained, not oxigen in the one receiver, and hidrogen in the other, but these two substances in each, in the exact proportion, in which they combine together to form water: for on passing the electric spark through the gasses collected in each receiver, separately, a detonation took place, the gasses entirely disappeared, and water was regenerated. The nature of the decomposition, which happened in each receiver, was obvious: the wire P, proceeding from the zinc side of the battery, being positive at the extremity p, and the water in the receiver operating as a conductor, the positively electric state was transmitted through the water to n, and then along the connecting wire $n\,p$ to p, which by this means became also positive; in like manner, the wire n connected with the copper side of the battery, being negative at the extremity n, and the negatively electric state being transmitted through the water to p, and then along the connecting wire $p\,n$ to the extremity n, this extremity became negative. There being thus a positive and a negative point in each receiver, the decompositions which took place differed in no respect from those which happen when the arrangement represented at fig. 1 is employed.

It

It now occurred to me, that every interruption of the circuit would afford a positive and a negative point; and that a series of decompositions might be procured, by following out the same arrangements in a succession of receivers. I therefore constructed an apparatus first with four interruptions in the circuit, and afterwards another with six, fig. 5; and in both cases, I obtained in each receiver, the elements of water in the proper proportions, in which they combine to form this fluid. The positive and negative points are marked in order.

Every interruption of the circuit affords a positive and negative point.

Though these experiments were perfectly decisive with regard to the effect produced by the connecting wire, and sufficiently calculated to unfold the real nature of the decomposition, to which it was subservient, I could not rest satisfied, till I had repeated an experiment, which Mr. Murray seems to adduce in confirmation of the imaginary transmission. I say, seems to adduce, for the experiment is stated with so little precision (considering the usual accuracy of this excellent chemist), that it is difficult to discover the real object, for which it is brought forward. After mentioning the experiment of Ritter, and adopting the conclusion which he deduced from it, he adds—"I have "found, too, that if a portion of quicksilver be interposed "between two portions of water, (which can be easily done "by filling the bent part of a siphon with quicksilver, and "putting water into each leg) on placing wires connected "with a galvanic trough in the separate portions of water, "gas arises from each wire*". In order to repeat the experiment of Mr. Murray, I constructed an apparatus, such as I have represented at fig. 6. *p a b n* represents the bent siphon, the opposite ends being introduced through two glass capsules, A, B, to which they were hermetically sealed at the bottom, *d, e*. Having filled the capsules and the bent siphon with water, I inverted over the extremities of the siphon two small receivers filled with water, through the ends of which I had previously passed the gold wires N *n*, P *p*, and to which they were sealed by melting the glass. I then connected the wire N *n* with the upper side of the battery, and P *p* with

An experiment of Mr. Murray's repeated.

Decomposition of water in separate vessels with quicksilver interposed.

* Murray's Chemistry, vol. I, p. 558.

with the zinc side, and immediately gas was disengaged at their respective extremities *n* and *p*. On examining the gasses obtained in the two receivers, the gas in the receiver connected with the negative side of the battery was hidrogen, and that in the receiver connected with the positive side, oxigen. This arrangement did not differ essentially from that represented at fig. 2; and the reason why the gasses are found separate is equally applicable to both.

Oxigen in one vessel and hidrogen in the other, when water only was employed:

I then removed the water out of the bent siphon, and supplied its place with mercury, confidently expecting, that the mercury (making allowance for its oxidable property) would operate precisely as the connecting wire in the arrangement represented at fig. 4. Accordingly, on connecting the wires N and P with the opposite sides of the battery, in a few seconds I perceived an oxidation of the mercury taking place at the point *p* of the bent siphon, which, as the wire P *p* was connected with the zinc side of the battery, was a positive point. Gas was copiously disengaged at the opposite extremity of the siphon, as well as from the points *n* and *p* of the connecting wires. After allowing the decomposition to go on during some minutes, I examined the gasses in the two receivers. The gas in the receiver over the capsule B exploded by the electric spark, and disappeared completely, while no effect whatever was produced by passing a succession of electric sparks through the gas in the receiver over A. I therefore introduced into this receiver as much oxigen, by measure, as was equal to half the bulk of the gas which it already contained, and which I had no doubt was pure hidrogen: I then passed the electric spark through the mixture, when an explosion took place, and both gasses completely disappeared.

but when mercury was interposed, this was oxided in one receiver, and left pure hidrogen, while the other receiver contained both oxigen and hidrogen.

Ritter therefore misled.

This experiment, therefore, so far from supporting the opinion of Ritter, shows, that he must have been misled by a partial view of the circumstances attending the decompositions, while it affords an additional illustration of what I have already stated with respect to a series of alternately positive and negative points at every interruption of the circuit.

Another experiment with an

Pursuing still farther the idea of this alternation of the electric states, I cemented to a glass rod a succession of small

small bits of gold wire, and having interposed them in that state, between the extremities *p* and *n*, fig. 1, of the two wires connected with the positive and negative sides of the battery, I observed, with pleasure, a considerable disengagement of gas taking place, at the same time, from each extremity of all the unconnected wires, which formed the galvanic circuit. Interrupted circuit.

Having thus pointed out the circumstances which misled Ritter and his followers, and established, beyond all doubt, the important fact of a positive and a negative point at every interruption of the circuit, it is almost unnecessary to observe, that the decompositions, which happen by employing the arrangement first suggested by that philosopher, admit of being explained on the same principles as the decomposition effected by introducing under the same receiver a positive and a negative point, proceeding immediately from the galvanic battery. The principle of the decomposition therefore the same in all cases.

I am, Sir, your most obedient servant,

ADAM ANDERSON.

Perth Academy,
Sept. 23, 1811.

VI.

Description of several new Varieties of carbonated Lime: by Mr. HAUY*.

THOSE problems, of which the object is to determine the varieties of a crystallization having a rhomboid for its primitive form, are susceptible of two solutions, which lead to the same figure by different laws of decrement. Mechanical division, by making known the position of the faces of the nucleus with respect to those of the secondary crystal, shows on which of these two laws the figure of a given crystal depends. Two laws of decrement for a rhomboidal nucleus, determinable by cleavage. In the course of a long time I had very seldom met with the two solutions at once in the same system of crystallization; but instances of this kind have been more numerous among the recent observations I have made Sometimes nature follows both.

* Journal des Mines, vol. XXIII, p. 49.

on

on the varieties of carbonate of lime, of which I have now 93 in my collection. I shall describe some of these, which realize the possibility of this double employ of the same figure with two different structures.

Trihexaedral carbonate of lime.

The trihexaedral carbonated lime, Pl. VI, fig. 7*, a specimen of which was presented me by Mr. Hericart de Thury, exhibits itself in the form of a regular hexaedral prism C C', terminated by two right hexaedral pyramids P ι. Three faces, P, of each pyramid, taken alternately, are parallel to those of the nucleus. The other three, designated by ι, which arise from a decrement by two rows in height on the lower angles of the nucleus, are inclined to the adjacent sides at the same angle as the preceding, namely 135°; so that the secondary rhomboid, which the union of these faces would produce if they existed alone, would be similar to the nucleus.

This result, which I have demonstrated in the geometrical part of my treatise, may be considered as the limit of all those, to which the double solutions I have spoken of lead; because it is that, in which, one of the two quantities expressing the decrement becoming 0, the solid answering to this term is the nucleus itself.

Ambiguous carbonate of lime.

In the ambiguous carbonated lime, fig. 10, the dodecaedron 9 9, which in this variety is combined with the inverse rhomboid *f f*, and the sides C C' of the regular hexaedral prism, is similar to the metastatic dodecaedron, vulgarly dogtooth spar; but it depends on a different law of decrement, of the kind of those I have called intermediate. This result requires a certain explanation to be well understood.

Common metastatic dodecaedron.

In the common metastatic dodecaedron, fig. 11, the least saliant edges answer to the faces of the nucleus, while the most saliant are turned toward its edges. I had inquired, when I wrote the geometrical part of my treatise, whether there were not a law of decrement capable of producing a secondary crystal similar to the metastatic, so that the edges turned toward the faces of the nucleus should be, contrary to it, the most saliant; and I found, that this result would take place from the intermediate decrement $\overset{5}{{}^{4}E}\;\overset{5}{{}^{4}B}\;{}^{1}D^{3}$.

* Fig. 8 represents the primitive form.

On

On the other hand, the common inverse rhomboid has its faces turned toward the superior edges of the nucleus: and, having also examined what law would give the same rhomboid, with its faces answering to those of the nucleus, I was led by calculation to the result expressed by $\overset{s}{e}$. — Common inverse rhomboid.

Let us suppose, that the common inverse rhomboid is combined in one figure with the common metastatic dodecaedron; it is evident, that its faces would answer to the most saliant edges of this dodecaedron: but in the variety before us, on the contrary, they answer to the least saliant edges. Now there are two different cases, in which this may take place: one is that in which the metastatic would result from the law $\overset{2}{D}$, and the inverse rhomboid from the law $\overset{s}{e}$; the other, that in which the metastatic would be produced by the intermediate decrement, and the rhomboid by the decrement E^1 ^1E. Mechanical division removes all ambiguity by proving, that the second is the case. The faces of the two solids combine, as I have said, with the sides of the hexaedral prism, from which we can derive no indication in favour of one structure, or of the other. — Combination of these. Structure of the present variety.

The stenonome carbonated lime, fig. 9, differs from that which I have described in my treatise under the name of subtractive by the addition of the facets ι and π. The former afford a fresh example of the law of decrement, which tends to produce a rhomboid similar to the nucleus. The faces π π exhibit a particular case, the possibility of which I had proved; namely that in which the decrement on B, fig. 8, taking place by two rows, would produce a dodecaedron, all the triangles of which, instead of being scalene as in the other cases, would become isosceles; that is to say, the dodecaedron would be composed of two right pyramids united base to base. In fact we should have a dodecaedron of this kind by prolonging the faces in question till all the others had disappeared. — Stenonome carbonate of lime.

The angle of 151° 2′ 42″, which measures the respective incidences of the faces of this dodecaedron, is exactly double the angle of smallest incidence of the faces of the nucleus, 75° 31′ 21″. These proportions between the angles — Proportions between the angles.

of the primitive form and those of the secondary crystals are not unfrequent in the varieties of carbonated lime.

Crystals formerly supposed.

From these examples it is seen, that results, which I had given as merely hypothetical, appear as descriptions by anticipation of so many products of crystallization, which existed in the bosom of the Earth without our knowledge.

VII.

Extract of a Letter from Dr. FRANCIS DELAROCHE *to* F. BERGER, *Esq.; on Radiant Heat and other Subjects. Communicated by the latter Gentleman.*

PARIS, *July the 17th*, 1811.

Phenomena of radiant heat.

IN my last two letters, I mentioned to you an inquiry into the phenomena of radiant caloric, which I commenced last spring, and of which the principal results are the following.

Radiant caloric, almost entirely divested of the faculty of traversing glass, when the substance that emits it is at less than 100° [212° F.] or even 180° [356° F.], acquires this property very manifestly, and independent of the light that may accompany it, in proportion as the temperature of the heated body is increased beyond this.

The rays emitted simultaneously by one and the same heated body differ from each other with respect to the faculty of traversing glass.

The quantity of radiant caloric emitted, or, to speak more properly, the quantity of caloric arriving at a distance in the radiant form is not proportional to the temperature of the heated body, as commonly supposed, but it is infinitely greater in proportion at high temperatures, than at lower.

Lastly, that the law of refrigeration established by Newton, though nearly accurate at low temperatures, is far from being so at high ones.

Phenomena of light.

Nothing very striking has occurred here in the sciences within these few months. Mr. Malus is still pursuing with success his inquiries concerning *polarised* light. Mr. Arago likewise is making some curious experiments on the same subject. Some, that he has lately made on the illu-

Illumination of the sun.

mination

mination of different parts of the solar disk, show, that the degree of illumination of the edges and of the centre is precisely the same, contrary to the opinion generally received. Mr. Clèment has very happily applied prof. Leslie's process for the formation of ice to the rapid and complete dessiccation of various animal and vegetable substances. He has also greatly improved the apparatus for evaporating liquids by the help of fire.

Dessiccation of animal and vegetable substances.

Evaporation.

VIII.

On Chemical Attraction. By MARSHALL HALL, *Esq.*

To W. NICHOLSON, Esq.

SIR,

CHEMICAL attraction is that force, by which the particles of matter are drawn towards each other. These particles are of two kinds; for they may be similar to each other, as in the same simple body, when they are termed homogeneous; or they may be dissimilar, as in a compound body, and are then denominated heterogeneous. From this distinction between the particles of material objects, a division of the attraction, which unites them, immediately flows. The force, which occasions similar particles to cohere, is called homogeneous attraction; dissimilar particles are united by heterogeneous attraction: the former is the cause of cohesion in *simple* bodies; the latter occasions combination between different bodies.

Chemical attraction.

Homogeneous and heterogeneous attraction.

But, beside these, philosophers have supposed, that a third order of particles, and of attraction, influences chemical actions. "Heterogeneous affinity urges heterogeneous particles toward each other, and of course is the cause "of the formation of *new integrant particles*, composed of "a certain number of heterogeneous particles. These new "particles afterward unite by cohesion, and form masses of "compound bodies *." In the words of Mr. Murray, "the "integrant particles are merely the smallest particles, into "which

A third order of particles and of attraction supposed.

* Thomson, ed. 3, vol. III, p. 408.

"which a substance can be resolved without decomposition. "The integral parts are united by the force of aggregation, "the constituent parts by chemical affinity *." Berthollet describes the force of cohesion of a compound, as that by which the integral parts are held together †.

This has led to errours in the general theory of chemical attraction.

It is the object of the following observations, to point out what I conceive to be an inaccuracy, in the opinion of compound integrant particles, and of the attraction by which they are supposed to be united; and especially to notice some errours, which have been introduced into the general theory of chemical attraction, by the adoption of this opinion.

Opinion of compound integrant particles hypothetical.

It is proper to premise, that the opinion itself of compound integral particles must be admitted to be hypothetical. We mix two substances together, and their particles unite in that manner, which constitutes chemical combination; but to say in what precise manner they unite, I apprehend to be impossible; that they first collect together to form particles of a new kind, and of a superior order, which unite by homogeneous attraction, is surely not very manifest. It is perhaps more probable, that chemical union is a less complicated operation. If a number of heterogeneous particles be mixed together, they assume respectively that situation, which their mutual attraction allots to them; every particle is probably attracted by every other; and of this attraction, combination and aggregation are equally the effects.

Combination and aggregation effects of the same cause.

Nor can the cohesion of a compound substance be attributed more to the agency of homogeneous, than of heterogeneous attraction; for if, in a compound, the particles be drawn towards each other, it is of no importance whether these particles be similar or dissimilar; the same effect, in point of cohesion, will be produced.

The contrary hypothesis improbable.

The account therefore usually given of the formation of the integrant particles of a compound, which unite by homogeneous attraction, or cohesion, is not only without proof, but, as I humbly conçeive, without probability. We shall however admit the opinion, and proceed to consider how it

* Murray, ed. 2, vol. 1, p. 63. † Researches, p. 38.

accords

accords with and explains the phenomena of chemical combination.

Berthollet, in his researches on this subject, has ascribed many phenomena to the operation of the homogeneous attraction, which unites integrant particles, or, as it is termed by him, cohesion. He considers it as a powerful cause in modifying combination; and especially, he attributes many of the results of complex affinity to its influence; he supposes, that Bergman's Tables do not represent the real order of the affinities of bodies, but rather, the degree of cohesion possessed by the compound when formed *.

Cohesion supposed to modify the action of chemical affinity.

The following illustration is given of the mode of operation of this force of cohesion. "If a solution of sulphate "of potash be mixed with muriate of lime dissolved in a "small quantity of water, the lime brought into contact "with the sulphuric acid will be more powerfully influenced "by the force of cohesion, than the potash. It is therefore "a force in addition to those which preexisted, and deter"mines the combination of the sulphuric acid with the lime, "and the precipitation of the new compound †."

Instanced in sulphate of potash and muriate of lime.

As this paragraph comprehends much of the doctrine of the influence of cohesion in modifying chemical union, it deserves particular notice, and it will be of advantage to make a few observations on it.

This instance examined.

It may be inquired, what is to be understood by the lime being brought into contact with the sulphuric acid? Is chemical contact or chemical union intended? It is difficult to determine this question. If chemical union be not intended by the word contact, it is improper to say, that the lime will be more powerfully influenced by the force of cohesion, than the potash; for muriate of lime is more soluble than sulphate of potash. Let us suppose, that chemical union is intended, and we shall still observe a manifest impropriety in the account of the influence of cohesion which follows.

The lime does not exert a stronger force of cohesion than the potash before decomposition has taken place.

It is said, that cohesion is a force, in addition to those which preexisted, and determines the combination of the sulphuric acid and the lime, and the precipitation of the new compound. Now it is to be observed, that this new force can only be exerted, when "the lime is brought into

A power that does not act till an effect has taken place cannot have produced the effect.

* See Researches, p. 106. † Ibid, p. 108.

"contact with the sulphuric acid;" how then does it afterward "determine the combination of the sulphuric acid "with the lime?" A power which is only evolved at the instant of the combination of two substances, cannot surely influence, or *determine* that combination.

Cohesion is a power, which is exerted between *integrant* particles only; in this instance between the integrant particles of sulphate of lime; it has no influence before their existence, and consequently cannot contribute to their formation, it cannot therefore be a power in addition to those which preexisted, so as in its operation to determine the combination of the sulphuric acid and the lime.

It appears to me, therefore, that Berthollet has attributed the formation of saline compounds to the active energy of a power, the very existence of which, according to his own definition, must be coeval with, and cannot precede and influence their formation.

Cohesion between the particles of a compound is a force that determines its formation.

Now it is to be observed, that the proposition, that cohesion in a compound is a force which determines the formation of that compound, is really a fact as well established as any in chemistry. For, "if all the decompositions ascribed "to complex affinities be investigated, it will be found, that "the prevailing affinity has been always ascribed to those "substances, which have the property of precipitating, and "of forming a salt, which can be separated by crystal-"lization *." The formation of these compounds, therefore, can scarcely be attributed to any other cause, than that which Berthollet alleges; namely, the operation of the attraction of cohesion in the compounds formed.

Combination and aggregation the joint effect of heterogeneous and homogeneous attraction.

On the contrary, that cohesion is exerted between compound integrant particles only, nay, the very existence of such particles, is entirely hypothetical. The former proposition is supported by an ample number of experiments; the latter, which is in contradiction to it, is merely matter of opinion. Experiment, which is the light of Nature, shows us, that that power, which we term the attraction of cohesion, does influence and determine the combination of those substances, or of those particles, which constitute a compound with much cohesion: but, as it has been shown, these

* Berthollet, Researches, p. 106.

particles

particles cannot be what are termed *integrant*; the constituent particles of a compound are therefore made to approach by the agency of the force of cohesion; just in the same manner as by chemical attraction. Where then is the distinction between these two powers? To me it appears, that there is no distinction whatever; but that, in fact, aggregation and combination are both the effects of the mutual attraction of heterogeneous particles. In the compound A B, each particle of A is probably attracted by every other particle of A, and by every particle of B. Now the first of these attractions is homogeneous; the second heterogeneous. It is therefore probable, that the particles of every compound unite and adhere by the agency of both these kinds of attraction; it is surely improper to assert, that they *unite* by the agency of one attraction, but *adhere* by the influence of the other.

It is proper to observe, that the change, which is here suggested with regard to our opinions of the attractions of cohesion and of combination, is not so singular as at first view it may be supposed to be. A change precisely analogous has been proposed, relative to the operation of affinity between two or more compounds. Formerly it was supposed, that, when two binary compounds, for example, are submitted to mutual action, the energy exerted in their union subsisted between the integrant or homogeneous particles of these compounds. A view of the subject, very different, has however been given by Berthollet. He supposes, that two compounds act on each other by an affinity *resulting* from the united energies of their constituent elementary particles. A change precisely similar is here suggested relative to the attraction of aggregation in a compound body. The prevailing opinion is, as it was formerly with respect to chemical affinity, that the attraction is exerted between compound particles. I suggest, that, as in affinity, the powers may be exerted between constituent and elementary particles. Both powers may with equal propriety be termed *resulting* attractions.

If this opinion concerning chemical attraction be correct, certain consequences will necessarily follow, which it may be proper to point out. 1st. Those compounds, the constituents

Consequences of this opinion.

tuents of which have the greatest affinity, will have the most cohesion; they will be the least soluble, and the first to crystallize on evaporation. 2dly. Bergman's Tables may still be considered as representing, in some degree at least, the affinities of bodies. 3dly. Berthollet's opinion, that, when two binary compounds are dissolved together, a quaternary compound is always formed by their union, will be in great measure invalidated. These consequences we shall endeavour to trace; so that, if the opinion we have stated be just, it may receive due confirmation, from the observation of the phenomena presented by chemical attraction.

Compounds that have the greatest affinity least soluble.

The first proposition is abundantly proved by the following observations of Berthollet, which deserve to be quoted a second time. "If all the decompositions ascribed to complex affinities be investigated, it will be found, that the prevailing affinity has been always ascribed to those substances, which have the property of precipitating, or of forming a salt, which can be separated by crystallization. For this reason it may be inferred *a priori*, from a knowledge of the solubility of salts which may be formed in a liquid, that those substances, which are least soluble, and most apt therefore to precipitate, will be found to be the same as those to which Bergman and other learned chemists have attributed the strongest affinity in their tables," &c.—See Researches, p. 106 et seq.

Sulphates of barytes and potash.

Barytes has a stronger affinity for sulphuric acid than any other base; it therefore decomposes all the sulphates. From the same energetic attraction the particles of sulphate of barytes cohere with more force, and it is found to be less soluble than the other sulphates. Thus we consider the forcible attraction, which subsists between sulphuric acid and barytes, as at once the cause of the decomposition of the sulphate of potash, and of the strong cohesion, and of the little solubility of the new sulphate. This account of the matter I think is perfectly just and reasonable, whereas we have shown the incongruity of the opposite opinion.

Bergman's Tables represent the real affinities of bodies.

We are now naturally led to consider our second proposition; that Bergman's Tables may still be considered as representing the real affinities of bodies. If Berthollet's opinion, that the decomposition and separation of salts arise from

from the influence of a power altogether distinct from chemical affinity, be correct, it is obvious, that Bergman's Tables merely represent the cases, in which this power is most energetic, and do not denote the order of affinity of the various substances comprehended in them: but if, on the contrary, it be proper to consider the force of cohesion in a compound as depending in equal measure on heterogeneous and homogeneous attraction, then it as obviously follows, that Bergman's Tables do indeed represent the order of affinities of the substances arranged in them.

It will be very evident, that this question is of great importance; and I hope what has been said, together with some observations connected with our third proposition, will render the view we have taken of it sufficiently probable. It is to be observed, that Bergman's Tables of Chemical Affinities may err from other causes; sufficient regard may not have been paid to the proportions, volatility, &c. of the substances; it is only in reference to the supposed influence of the attraction of aggregation of the compound, that they may still be regarded as expressing real affinities. The force of cohesion of the individual constituents of a compound will influence the formation and the state of aggregation of that compound; for I suppose, that the force, which causes the particles of a simple body to approach each other, is not destroyed or suspended, when that body enters into combination. Yet still, as this degree of cohesion between the particles of any body may be considered as a property of that body, Bergman's Tables may be considered as denoting the order of affinity in any number of bodies endowed with such properties.

Two binary compounds may exist together in solution, not forming a quaternary compound.

Our third proposition has been discussed with much ability by Mr. Murray, who considers it as equally probable *a priori*, that two binary compounds should exist together in solution, as that they should unite to form a quaternary one. He adds, "it is very doubtful, whether Berthollet "has not extended too far the principle, on which his theory "of complex affinity is established."

Or why is not the quaternary compound obtained by evaporation?

If a quaternary compound be formed on the solution of two binary ones in water, it is a natural question, why is not this quaternary compound obtained by evaporation? Here the

the influence of the attraction of aggregation is alleged as the cause of the decomposition of the quaternary compound, and of the formation of the two binary compounds obtained by crystallization; but in this instance the same inconsistency occurs, as has been pointed out, in considering the formation of sulphate of lime on the same principle. A power developed at the instant of the formation of a compound is represented as the *cause* of the formation of that compound.

Hence it appears, that, supposing the solution to contain a quaternary compound, no reason can be given, why this compound is not obtained, on the evaporation of the solvent; the inference therefore must be, that such a compound did not exist in solution; but that the substances dissolved are really binary compounds, such as we obtain them.

And even on the opposite supposition, that cohesion does not at all depend on heterogeneous affinity, I think the same inference might be deduced. If a solution contain a quaternary compound, which becomes two binary compounds on evaporation; this change must take place, at the instant of crystallization: but why it does take place at that instant, we are not told; there is no new power called into action; because Berthollet's notion of the force of cohesion is, that it is a power not only when apparently effective, but also when it appears to be entirely overcome. The question then remains, if the solution be that of a quaternary compound, what is the reason, that, at the point of crystallization it is decomposed, and that two binary compounds are obtained on evaporation?

Influence of volatility.

Speaking of the influence of volatility on affinity, Berthollet remarks, that "heat, by increasing the volatility of "a substance, enfeebles its combination, and this cause is not "less efficient in complex, than in elective affinities: it is a "force added to those already in action, and which determines "the union and separation of *those substances*, which are most "disposed to form a *volatile compound*." Still the same objection may be urged against this observation, as was offered to the alleged influence of cohesion in effecting combination.

That of a resulting compound cannot influence its formation.

The volatility of a compound can have no influence on the formation of that compound; combination, in every instance, depends on the properties of the constituents, and not

not on the substance constituted. If the constituent parts of a compound be volatile, this volatility will be increased by heat, and their union will probably be promoted; but it is of no importance whether the compound be volatile or fixed when formed.

Causes of limits to chemical combination according to Berthollet.

Berthollet has explained in a far more satisfactory manner than former chemists the causes of some of the limits, which are observed in chemical combination; he has pointed out the influence of quantity, cohesion, volatility. But there still remains considerable difficulty in accounting for these limits on many occasions; as in the following instance, where condensation is considered as the cause, determining the proportion of the constituent parts of the compound, and affording the limit to combination.

Great condensation.

"When, in the *progress* of combination, the result in any "part of it is great condensation; this, by the obstacle it "may oppose to the exertion of affinity, or even by the "greatness of the condensation withdrawing the product "from the sphere of action, may limit the combination to "that point, or to the proportion at which this effect is "greatest; or if by particular circumstances this is over- "come, in the farther *progress* of combination it may hap- "pen; and in this way, compounds in two or three deter- "minate proportions may be formed.*" I am not certain, with what propriety we speak of the "progress of combination"; because I do not know, that we have any reason to believe it to be *progressive*; progressive in the sense implied, in relation to proportions. If oxigen be combined with hidrogen, the compound is established in a certain determinate proportion; nor have we any reason for supposing, that the combination ever took place in any other proportion; much lefs can we presume, that the proportion of one or the other increases progressively, until the occurrence of a considerable condensation puts a period to the progress of combination, and determines the proportions of the compound. The cause of the condensation is likewise unknown; unless it be attributed to the formation of new integrant particles, possessed of a new attraction; which is

Combination probably not progressive in respect to proportions.

* Murray, vol. 1. p. 105.

Mr. Dalton's views of proportion.

an hypothesis, that cannot be admitted. On the subject of proportion, Mr. Dalton's views give the happiest explanation; but they must be acknowledged to be hypothetical. The particles of bodies are objects of science, and not of sense; yet we speak of them, as if they were as palpable and visible as the masses they compose.

Union of gasses on the application of heat, and by compression.

I shall conclude these observations by an allusion to two instances of combination, which have not, I think, been very satisfactorily explained. The first is the union of gasses on the application of heat; the second, the same combination by means of compression. They are both explained on the mechanical principle of an approximation of the particles, occasioned by the compression; but it is to be remembered, that, as both operations may be attended by compression, they are also both attended by an increased temperature. May not the heat have some more immediate operation in attracting these combinations, than is supposed: I believe the pressure, which causes the gasses to combine, requires to be applied very suddenly, so that a due quantity of heat may be evolved. It is requisite, that the application of heat to inflame gasses be applied suddenly, so as to ensure a considerable compression? The determination of both these questions by experiment would be extremely interesting. The influence of compression is seldom or never resorted to in pneumatic experiments; although it would undoubtedly prove an agent of considerable power in promoting combination. I am, Sir,

Yours respectfully,

MARSHALL HALL.

University, Edinburgh,
September the 25th, 1811.

IX.

On the Horticultural Management of the Sweet or Spanish Chestnut-tree. By the Right Hon. Sir JOSEPH BANKS, *Bart, K. B. &c.**

Chestnuts grafted on the continent,

IN all the northern parts of Europe, where chestnuts are used for food, the practice of grafting the trees that bear

* Trans. of the Horticultural Society, vol. I, p. 140.

them

them has been known from time immemorial; the wild or ungrafted chestnut is called in french châtaignier, the grafted or cultivated sort maronnier.

Though the grafting of chestnuts has been little, if at all used in this part of the island, it is not an uncommon practice in Devonshire, and other western counties. The nurserymen there deal in grafted chestnut trees, and the gentlemen have no doubt introduced them into their gardens. *and in the west of England.*

About sixteen years ago, sir William Watson sent some of these grafted trees from Devonshire to Spring Grove, with an assurance, that the fruit would be plentiful and good. They were at first neglected and ill treated, owing to the disinclination most gardeners have to the introduction of novelties, the management of which they are unacquainted with: it was therefore six or seven years before they began to bear fruit.

Since that time, as the trees have increased in size, the crop has every year become more abundant; last autumn the produce, though they are only six in number, was sufficient to afford the family a daily supply from the beginning of November till after Christmas. The nuts are much smaller than the Spanish imported fruit, but they are beyond comparison sweeter to the taste. The crops are little subject to injury, except from very late frosts. The trees are in general covered with blossoms to a degree, that retards their annual increase. They are now so low, that a part of the crop is gathered from the ground, and the remainder by a step-ladder. They require no care or attendance on the part of the gardener, except only the labour of gathering the fruit. Most people prefer the taste of the fruit to that of the imported, but there can be no doubt, that, when the usage of grafting chestnuts becomes common in this country, grafts of all other sorts will in due time be procured from the continent. *The English much sweeter than the Spanish.*

The kernels of these chestnuts, and of all others ripened in England, are more liable to shrivel and dry up than those imported, owing to a deficiency of summer heat in our climate to mature the fruit; this must be guarded against by keeping the nuts always in a cool place, rather damp than *Mode of keeping them.*

dry;

dry; the vessel best suited to preserve them is an earthen ware jar with a cover, this will not only keep them cool, but it will restrain the loss of moisture without entirely preventing perspiration, and thus endangering the loss of vitality, the immediate consequence of which is the appearance of must and mouldiness."

X.

On Potatoes. By THOMAS ANDREW KNIGHT, *Esq. F. R. S. &c.**

Suggestions with regard to the culture of potatoes

IN the Horticultural Transactions of 1807†, I have described a method of cultivating early varieties of the potato, by which any of those, which do not usually blossom, may be made to produce seeds, and thus afford the means of obtaining many other early varieties. I also offered a conjecture, that varieties of moderately early habits, and luxuriant growth, might be formed, which would be found well adapted to field-culture, and be ready to be taken from the soil in the end of August, or the beginning of September, so that the farmer might be allowed ample time to prepare the same ground for a crop of wheat. I am now enabled to state, that the success of the experiment has in both cases fully answered every expectation that I had formed.

have succeeded in practice.

The tubers and blossoms produced by the same sap.

The facts that I have stated in the Horticultural Transactions of 1807, and more fully in the Philosophical Transactions, are, I believe, sufficient to prove, that the same fluid, or sap, gives existence alike to the tuber, and the blossom and seeds, and that whenever a plant of the potato affords either seeds or blossoms, a diminution of the crop of tubers, or an increased expenditure of the richness of the soil, must necessarily take place. It has also been proved by others, as well as myself, that the crop of tubers is increased by destroying the fruit-stalks and immature blossoms as soon as they appear; and I therefore conceived, that considerable advantages would arise, if varieties of suf-

Large crops from varieties producing no blossoms.

* Trans. of the Hort. Soc. vol. I, p. 187. † See Journal, vol. XIX, p. 97.

ficiently

ficiently luxuriant growth, and large produce, for general culture, could be formed, which would never produce blossoms.

I have since had the gratification to find, that such are readily obtained, by the means which I have detailed, and I am disposed to annex more importance to the improvement of our most useful plants, than any writer on agriculture has hitherto done; because whatever increased value is thus added to the produce of the soil is obtained without any increased expense or labour, and therefore is just so much added to individual and national wealth.

Peculiar advantages of improved varieties of plants.

I formerly supposed that all varieties of the potato, which ripened early in the autumn, would necessarily vegetate early in the ensuing spring, and could therefore be fit for use only during winter; but I have found that the habit of acquiring maturity early in the autumn is by no means necessarily connected with the habit of vegetating early in the spring; and therefore by a proper selection of varieties, the season of planting crops, for all purposes, may be extended from the beginning of March, nearly to the middle of May, and each variety be committed to the soil exactly at the most advantageous period.

Potatoes that ripen early in autumn do not necessarily vegetate early in spring:

A variety, however, which does not vegetate till late in the spring, and which ripens early in the autumn, cannot, I conclude, particularly in dry soils and seasons, afford so large a produce as one which vegetates more early: I, nevertheless, obtained so large a crop from one which vegetates remarkably late in the spring, and ripens rather early in the autumn, that I was induced to ascertain, by weighing, to what the produce would have amounted, had the crop extended over an acre, and I found, that it would have exceeded 21 tuns, 11 cwt. 80 lb*.

but most produce to be expected from those that vegetate early.

Produce.

In this calculation the external rows, which derived superior advantage from air and light, were excluded; and no more manure, or culture, than is usually given, had been employed; for the crop was not planted with any intention of having it weighed: the wet summer was, however, very favourable.

* 48352 lbs.

I am

Calculation of the proportion of food obtained from ground in potatoes,

I am not acquainted with the common amount of the weight of a good crop of potatoes, upon an acre of ground in a favourable soil, when well manured and cultivated; but I am confident, that it may generally be made to exceed 20 tons, by a proper selection of varieties: and if four pounds of good potatoes afford, as is generally supposed, at least as much nutriment as one pound of wheat, the produce of an acre of potatoes, such as I have described, is capable of supporting as large a population as eight acres of wheat; admitting the calculation of Mr. Arthur Young, that the average produce of an acre of wheat is 22½ bushels*: and as an acre of wheat will certainly support as large a number of people as five acres of permanent pasture, it follows that an acre of potatoes affords as much food for mankind as forty acres of permanent pasture: an important subject for consideration, in a country where provisions are scarce and dear, and where so high bounties on pasture are paid in the form of taxes on tillage, that the extent of permanent pasture is certainly and consequently increasing; and it must increase, under existing circumstances; for it pays a higher rent to the landlord, and relieves the farmer from much labour, anxiety, and vexation.

wheat,

and pasture.

Prevention of blossom would pay the rent of the ground.

To what extent a crop of potatoes will generally be increased by the total prevention of all disposition to blossom, the soil and variety being, in all other respects, the same, it is difficult to conjecture; but I imagine, that the expenditure of sap in the production of fruit stalks and blossoms alone would be sufficient to occasion an addition, of at least an ounce, to the weight of the tubers of each plant; and if each square yard were to contain eight plants, as in the crop I have mentioned, the increased produce of an acre would considerably exceed a tun, and of course be sufficient, in almost all cases, to pay the rent of the ground.

Varieties suited to Ireland not so to England.

I do not know how far other parts of England are well supplied with good varieties of potatoes; but those cultivated in this part of the island are generally very bad. Many of them have been introduced from Ireland, and to that climate they are probably well adapted; for the Irish planter is secure from frost from the end of April nearly to the end

* 1440 lbs.

of

of November: but in England the potato is never safe from frost till near the end of May; indeed I have seen the leaves and stems of a crop, in a very low situation, completely destroyed as late as the 13th of June, and they are generally injured before the middle, and sometimes in the firſt week of September.

The Irish varieties, being excessively late, are almost always killed by the frost while in full blossom; when, omitting all consideration of the useless expenditure of manure, it may justly be questioned whether the tubers of such plants, being immature, can afford as nutritive, or as wholesome food, as others which have acquired a state of perfect maturity.

Importance of obtaining varieties suited to this country.

The preceding statement will I trust point out to the Horticultural Society the importance of obtaining improved varieties of the potato, and I believe no plant existing to be more extensively capable of improvement, relatively to the climate of England; and if practical evidence were wanted to prove the extent, to which the culture of the potato is calculated to increase and support the population of a country, Ireland most amply affords it; where population has increased among the catholic poor, with almost unprecedented rapidity, within the last twenty years, under the pressure of more distress and misery, than has perhaps been felt in any other spot in Europe.

Remarks on the curl.

I shall conclude my present communication with some remarks upon the origin and cure of a disease, the *curl*, which a few years ago destroyed many of our best varieties of the potato; and to the attacks of which every good variety of the potato will probably be subject.

Origin of the disease.

I observed that the leaves of several kinds of potatoes, which were dry and farinaceous, that I cultivated, produced curled leaves; while those of other kinds, which were soft and aqueous, were perfectly well formed; whence I was led to suspect, that the disease originated in the preternaturally inspissated state of the sap in the dry and farinaceous varieties. I conceived, that the sap, if not sufficiently fluid, might stagnate in, and close, the fine vessels of the leaf during its growth and extension, and thus occasion the irregular contractions, which constitute this disease: and this conclusion

conclusion, which I drew many years ago, is perfectly consistent with the opinions I have subsequently entertained, respecting the formation of leaves. I therefore suffered a quantity of potatoes, the produce almost wholly of diseased plants, to remain in the heap, where they had been preserved during winter, till each tuber had emitted shoots of three or four inches long. These were then carefully detached, with their fibrous roots, from the tubers, and were committed to the soil; where having little to subsist upon, except water, I concluded the cause of the disease, if it were the too great thickness of the sap, would be effectually removed; and I had the satisfaction to observe, that not a single curled leaf was produced; though more than nine tenths of the plants, which the same identical tubers subsequently produced, were much diseased.

Experiment to prove the truth of the theory.

In the spring of 1808, Sir John Sinclair informed me, that a gardener in Scotland, Mr. Crozer, had discovered a method of preventing the curl by taking up the tubers before they are nearly full grown, and consequently before they become farinaceous. Mr. Crozer, therefore, and myself appear to have arrived at the same point by very different routes; for by taking his potatoes, while immature, from the parent stems, he probably retained the sap nearly in the state to which my mode of culture reduced it. I therefore conclude, that the opinions I first formed are well founded; and that the disease may be always removed by the means I employed, and its return prevented by those adopted by Mr. Crozer.

Prevention of the disease.

I sent to the Board of Agriculture the substance of the preceding remarks on the origin of the curl, in the year 1808; but I do not know whether that account has been published, or not.

Downton,
January 21, 1810.

XI.

XI.

A remarkable analytical Anomaly respectfully submitted to the Consideration of Mathematicians.

To Mr. NICHOLSON.

SIR,

IN your number for August last, I published a short paper on the defective algorithm of imaginary quantities, which has not at present been honoured by the remarks of any of your correspondents, though the importance of the subject seems to demand the attention of every advocate for the introduction of these expressions into mathematical investigations: and as I am extremely desirous to have the opinion of analysts on this subject, and particularly those, who in their writings have maintained the legitimacy of results, in cases where imaginary quantities have been nearly the only instruments employed in obtaining them; I am induced, in order to draw a reply from those quarters, to consider the same under rather a different point of view. For which purpose let us assume the two following expressions: On the algorithm of imaginary quantities.

$$\sqrt[3]{-2+11\sqrt{-1}}+\sqrt[3]{-2-11\sqrt{-1}}=-4$$

$$\sqrt[3]{-\tfrac{1}{2}+\tfrac{1}{2}\sqrt{-3}}+\sqrt[3]{-\tfrac{1}{2}-\tfrac{1}{2}\sqrt{-3}}=-1{\cdot}87938$$

which equalities may be verified either by the developement of the above expressions into series, or by the solution of the equations of which they are the roots, according to Cardan's rule, viz.

$$x^3-15x=-4$$
$$x^3-\ \ 3x=-1$$

Now let us square these formulæ at full length, and by precisely the same steps. Then we shall have the following operations:

$$(A)\ldots\left(\sqrt[3]{-2+11\sqrt{-1}}+\sqrt[3]{-2-11\sqrt{-1}}\right)^2=$$

$$\sqrt[3]{(-2+11\sqrt{-1})^2}+2\sqrt[3]{(-2+11\sqrt{-1})\times(-2-11\sqrt{-1})}$$

$$+\sqrt[3]{(-2-11\sqrt{-1})^2}.$$

Again

$$([illegible] -\tfrac{1}{2}+\tfrac{1}{2}\sqrt{-3}+\sqrt[3]{-\tfrac{1}{2}-\tfrac{1}{2}\sqrt{-3}}\Big)^2=$$

$$\sqrt[3]{(-\tfrac{1}{2} [illegible]}+ [illegible] \sqrt{-3})(-\tfrac{1}{2}-\tfrac{1}{2}\sqrt{-3})}+$$

$$\sqrt[3]{(-\tfrac{1}{2} [illegible]}$$

[illegible] rd step by step the same in both [illegible] ntinue the same parallelism of [illegible]

$$\left.\begin{array}{l}-2+11\sqrt{-1}\\-2+11\sqrt{-1}\end{array}\right\}\text{ the square of }(-2+11\sqrt{-1})$$

$$4-44\sqrt{-1}-121=-117-44\sqrt{-1}=(-2+11\sqrt{-1})^2$$

$$\left.\begin{array}{l}-2+11\sqrt{-1}\\-2-11\sqrt{-1}\end{array}\right\}\text{ the prod. of }(-2+11\sqrt{-1})\,(-2-11\sqrt{-1})$$

$$4+121\sqrt{-1}=125$$

$$\left.\begin{array}{l}-2-11\sqrt{-1}\\-2-11\sqrt{-1}\end{array}\right\}\text{ the square of }(-2-11\sqrt{-1})$$

$$4+44\sqrt{-1}-121=-117+44\sqrt{-1}=(-2-11\sqrt{-1})^2$$

and consequently our square (A) becomes

$$\sqrt[3]{-117-44\sqrt{-1}}+2\sqrt[3]{125}+\sqrt[3]{-117+44\sqrt{-1}}=$$

$$\sqrt[3]{-117-44\sqrt{-1}}+10+\sqrt[3]{-117+44\sqrt{-1}}$$

Again,

Again, to square our second formula (B)

$$\left.\begin{array}{l} -\frac{1}{2}+\frac{1}{2}\sqrt{-3} \\ -\frac{1}{2}+\frac{1}{2}\sqrt{-3} \end{array}\right\} \text{the square of } \left(-\frac{1}{2}+\frac{1}{2}\sqrt{-3}\right)$$

$$\frac{1}{4}-\frac{1}{2}\sqrt{-3}-\frac{3}{4} = -\frac{1}{2}-\frac{1}{2}\sqrt{-3} = \left(-\frac{1}{2}+\frac{1}{2}\sqrt{-3}\right)^2$$

$$\left.\begin{array}{l} -\frac{1}{2}+\frac{1}{2}\sqrt{-3} \\ -\frac{1}{2}-\frac{1}{2}\sqrt{-3} \end{array}\right\} \text{the product of } \left(-\frac{1}{2}+\frac{1}{2}\sqrt{-3}\right)\left(-\frac{1}{2}-\frac{1}{2}\sqrt{-3}\right)$$

$$\frac{1}{4}+\frac{3}{4} = 1$$

$$\left.\begin{array}{l} -\frac{1}{2}-\frac{1}{2}\sqrt{-3} \\ -\frac{1}{2}-\frac{1}{2}\sqrt{-3} \end{array}\right\} \text{the square of } \left(-\frac{1}{2}-\frac{1}{2}\sqrt{-3}\right)$$

$$\frac{1}{4}+\frac{1}{2}\sqrt{-3}-\frac{3}{4} = -\frac{1}{2}+\frac{1}{2}\sqrt{-3} = \left(\frac{1}{2}-\frac{1}{2}\sqrt{-3}\right)^2$$

And consequently our square (B) becomes

$$\sqrt[3]{-\frac{1}{2}-\frac{1}{2}\sqrt{-3}} + 2\sqrt[3]{1} + \sqrt[3]{-\frac{1}{2}+\frac{1}{2}\sqrt{-3}} =$$

$$\sqrt[3]{-\frac{1}{2}-\frac{1}{2}\sqrt{-3}} + 2 + \sqrt[3]{-\frac{1}{2}+\frac{1}{2}\sqrt{-3}}$$

Thus far likewise we have proceeded step by step in both operations.

And since our first formula is equal to -4, and our second to $-1{\cdot}87938$; the square of the former ought to be equal to $4^2 = 16$, and the latter to $(1{\cdot}87938)^2 = 3{\cdot}532069$; that is we ought to find the following equalities obtain: viz.

$$\text{(A)}\ldots\sqrt[3]{-117-44\sqrt{-1}} + 10 + \sqrt[3]{-117+44\sqrt{-1}} = 16$$

$$\text{(B)}\ldots\sqrt[3]{-\frac{1}{2}-\frac{1}{2}\sqrt{-3}} + 2 + \sqrt[3]{-\frac{1}{2}+\frac{1}{2}\sqrt{-3}} = 3{\cdot}532069$$

Or by transposing 10 and 2

$$\text{(A)}\ldots\sqrt[3]{-117-44\sqrt{-1}} + \sqrt[3]{-117+44\sqrt{-1}} = 6$$

$$\text{(B)}\ldots\sqrt[3]{-\frac{1}{2}-\frac{1}{2}\sqrt{-3}} + \sqrt[3]{-\frac{1}{2}+\frac{1}{2}\sqrt{-3}} = 1{\cdot}532069$$

 Now

On the algorithm of imaginary quantities.

$$\sqrt[3]{-117-44\sqrt{-1}} = 3-4\sqrt{-1}$$

and $$\sqrt[3]{-117+44\sqrt{-1}} = 3+4\sqrt{-1}$$

as will be found by involution.

And consequently their sum is equal to 6 as it ought to be, and we may therefore fairly conclude, that we are right in our operation on the first formula. But with regard to our second expression, it is the same as that with which we begun, and is therefore equal to 1·87938, and not 1·532069, as it ought t… had we been correct in the operations on the … and hence we may conclude, with equal certai… me mistake has crept in unob- rved in the latter cas… ithstanding we have proceeded by … allel … in … ples.

… hat I have to propose to mathe- …

1. … errours in the operation on the latter form …

2. How… guarded againſt in other cases?

The latter of these questions is equally important with the former; as there are various other formulæ of a similar description, which, should they arise in any investigation, when we have not the means of checking the result as in the examples above, much uncertainty must necessarily attend the conclusions thence deduced.

The manner in which I have introduced these questions may appear somewhat novel in the present day, but it was not uncommon at the time when the sciences were most successfully cultivated in this country, and when they were making those rapid advances, which have immortalized the names of several distinguished English mathematicians and philosophers.

I have only now to observe, that, should no answer appear to these questions within three months, I will then, through the medium of your Journal, publish my explanation; but I am not without hopes of seeing the subject elucidated by a more able hand than, Sir,

Your obedient Servant,

MATHEMATICUS.

XII.

On the Migration of Swallows: by Dr. TRAILL. *Read before a Literary and Philosophical Society established at Derby, Sept. the 17th, 1808, of which Dr.* TRAILL *is a Corresponding Member.*

To Mr. NICHOLSON.

SIR,

YOUR correspondent Mr. Forster having solicited information on the subject of the migration of swallows, Dr. Traill was induced to request, that the following paper, after having been read to the Derby Society, might be transmitted to you for publication. In compliance with that wish it is herewith enclosed; and, I have no doubt, will be considered as an interesting contribution to this curious branch of natural History. Migration of swallows.

I am, Sir,

Your very obedient Servant,

Derby. CHARLES SYLVESTER.

Extract from the Logbook of the Ship Jane of Lancaster.—Captain JOHN THOMSON.

On the 17th of May, 1807, in latitude 51° 42′ north; longititude 21° 44′ west. Pleasant clear weather. Wind W. N. W.

18th. Pleasant clear weather. Light airs and calms. Wind varying from S. E. to E. N. E. Lat. D. R. 52° 6′ N.; long. 21° 44′ W.

19th. Steady breeze from E. S. E. Some showers of rain, and foggy weather for the most part of this day. Lat. D. R. 52° 11′ N.; long. 21° 16′ W.

20th. Strong breezes, varying from S. to S. E. Foggy weather. About 4 p. m. several *martins and swallows appeared about the two ships*. At 8 p. m. collected to a large covey; many of which pitched on different parts of this ship, and allowed themselves to be taken up by the seamen. At daylight in the morning found many of them dead in the mizen Martins and swallows lighting on two ships in the Atlantic in May.

mizen top, channel bends, and on deck. Lat. D. R. 52° 33′ N. Long. 20° 21′ W.

21st. Continues foggy, attended with rain. Wind mostly from south-eastward. In the course of the day great numbers of the swallows and martins were taken by the seamen; and the cats and dog brought many of them. A great many had pitched in different parts of the ship; and all or the greatest part found dead in the morning.

Remarks by Dr. Traill.

The intelligent seaman, who made this extract from his logbook at my request, was then on his voyage from the West Indies. He has been many years captain of a ship in the West India trade from Lancaster, and from this port. I know him to be a man of probity and veracity; and his account was confirmed by some of the mariners of the ship then in company, with whom I conversed.

The circumstances chiefly to be attended to in the narration, are:

They were apparently on their passage from Africa to the north, not blown from the land by a storm.

1. The weather, previously, was not so boisterous as to countenance the idea, that the swallows were forced by a tempest from the nearest shore; and the general direction of the wind was not unfavourable to the supposition of their having been aided by it, in their passage from the coast of Africa, where they were observed by the celebrated, but unfortunate, Adanson, to arrive in the winter.

2. The season of the year is favourable to the idea of their migration from the coast of Africa for the north of Europe. They alighted on the ships about the time that swallows begin to appear in Britain, to which they were probably proceeding; and it should not be forgotten, that about this time of the year swallows are seen to quit the coast of Senegal, and other parts of Africa.

3. The debility of these birds, which permitted them to fall an easy prey to the cats and dog; their suffering themselves to be caught by the seamen; and their being very lean, as I was informed was the case by those who examined them in the two ships, seem to show, that they had made a long voyage, and not, that they had been accidentally driven by a gale, from the neighbouring shores of Britain

Britain and Ireland. Indeed, considering the great strength of wing, and velocity, of the swallow tribe, it must have been a tremendous gale that could drive them off the land: but, the previous weather was nothing boisterous, and captain Thomson experienced little more than a steady breeze.

4. The great number of these birds is another argument against the supposition of their having been carried to sea by a storm. Such instances in solitary birds of weak wing are not uncommon. I once caught a golden crested wren (*motacilla regulus*, Lin.) in the shrouds of a vessel, when driven off the coast of Scotland by a sudden tempest; but instances of large flocks of birds, so strong and active as the swallow tribe, becoming the sport of the winds, are certainly very uncommon, even when the weather has been tempestuous.

5. Captain Thomson expressly mentions both swallows and martins; and he stated to me, that they differed in size. Hence, there were, at least, two species of swallows observed by him. As he does not pretend to the character of a naturalist, perhaps, there were not only the chimney swallow, or *hirundo rustica*, and martin, or *h. urbica*, but the swift, or *h. apus*, and even the sand swallow, or *h. riparia*. This account, at least, supplies, in some degree, an omission of Mr. Adanson; who, in his interesting observations on the appearance of swallows in Africa, has omitted to state what species he observed there, or whether he observed more than one kind of swallow.

There were at least two species.

The preceding extract affords, in my opinion, another argument to prove the annual migration of swallows. That swallows sometimes have been found dormant, in the winter season, in cold climates, I am not disposed to deny. But had a bird so common with us generally remained here all the winter in a dormant state, we, probably, should have discovered it more frequently than has ever been pretended. I will even admit, that swallows have been found concealed amid rushes, by the banks of rivers, in this state: but that they have ever been discovered alive at the bottom of pools and rivers, or otherwise excluded from the access of atmospheric air, we must be permitted to doubt, till it is proved, that the respiratory organs of swallows differ

Their being found under water very questionable.

differ from those of other birds; or, that atmospheric air unnecessary to the life of dormant animals. The extraordinary suspension of most of the living functions of animals of this class is a subject of great physiological importance and curiosity; and deserves to be more fully investigated. But the claim of the swallow to an unusual structure of the organs of respiration is completely overturned by the dissections of the celebrated John Hunter. In the alleged cases of the submersion of swallows we must make allowance for the credulity, or inaccuracy, of observers; and I think it would not be difficult to refer almost all such alleged facts to one or other of these heads.

No unusual structure of the organs of respiration in swallows.

Liverpool. THOMAS STEWART TRAILL.

XIII.

Account of the Appearance of a Luminous Meteor: by Professor Pictet*.

Luminous meteor seen at Geneva.

THE 15th of this month, about half after eight in the evening, a luminous meteor was seen at Geneva, in the N. N. W. part of the sky, which was pretty clear where the meteor appeared, though there were clouds in other parts, and the phenomenon itself, toward the end of its appearance, was obscured by a cloud. The appearance was so sudden, that those of the spectators, who were looking another way, at the first moment supposed the light it gave, which was sufficiently vivid to cause a shadow, though it was still twilight, to be the effect of a flash of lightning. We have endeavoured to collect all the particulars respecting the circumstances of the phenomenon, that we could obtain from eyewitnesses of it. Among these may be distinguished five students of the academy, of the faculty of sciences, who happened to be walking together, and not only saw, but made their observations on this phenomenon, which they afterward committed to writing. These, except the noise, which was heard only by them, agree with all those, that have been communicated to us by others with less precision. The following are their words.

* Bibliothèque Britannique, for May, 1811, p. 105.

"The

"The 15th of this month, at 35 minutes after eight in the evening, we heard a whizzing sound in the north-west. A sudden flash of light caused us to turn our heads, and we saw a kind of serpent of fire, which appeared to us four or five degrees in length. It was bent back at the west end, so as to approach the figure of the letter S; it then spread out in the lower part; after which it assumed the shape of a horseshoe, and nearly of a parabola. At the end of seven or eight minutes, according to our watches, a cloud concealed it from our eyes, at the moment when it appeared to advance very slowly toward the west. Its brightness diminished every instant; and just at the time of its disappearing we no longer perceived any thing but two very bright points, one at the extremity of the lower branch of the parabola, the other on the same branch nearer the summit of the curve. As to its height we can say nothing precise, as we had no instrument with us adapted for measuring angles: but to the eye it appeared twice the height of mount Jura." Description of it.

One of the eye-witnesses of this phenomenon*, who observed it with a small telescope, remarked, that the most luminous part was not homogeneal, or continuous, but composed of distinct and separate particles. Composed of luminous points.

A fortunate circumstance enabled us to determine with tolerable precision the important circumstance of the apparent height of the meteor, which was for a long time nearly stationary. Two of its observers†, whom we consulted, remarked that the meteor, seen from a spot which they easily found again, grazed the summit of a certain tree, which even concealed part of its light: We afterward measured from the spot of observation the angle of altitude of this tree, and its azimuth. This altitude, and consequently that of the meteor, was eighteen degrees; and its azimuth was precisely in the direction of the magnetic meridian, which at present at Geneva is 20° 15′ N. W. This direction passes nearly through the zenith of the towns of Gray, Langres, Chaumont, Vitry, Chalons sur Marne, Rheims, Valenciennes, and Bruges. Its apparent height. Its azimuth.

* Mr. Trembley, nephew of the celebrated naturalist.

† Mr L'huilier, professor of mathematics in the academy; and Mr. Galland, student in the faculty of divinity.

We

It was seen at Paris.

We have learned by the public papers, that this meteor was seen at Paris; but it is not said in what direction. Supposing it to have been seen due east, the intersection of this azimuth with that observed at Geneva would point out the place of the meteor in the region of Vitry, Chalons, and Bar sur Ornain, about seventy leagues in a straight line from Geneva: a distance which, with its apparent observed height, and taking into account the effect of the sphericity of the Earth, would place the meteor about the actual height of twenty-four leagues and half.

Estimation of its real height.

The supposition we have made, for want of observations, may serve as a guide to those, who remarked nearly the azimuthal direction of the phenomenon, and would form an idea of its absolute height. It must have been less than 24½ leagues, if the meteor were seen in a direction to the southward of east; and on the contrary so much more, in proportion as it appeared more to the north of the perpendicular to the meridian of Paris.

Above our atmosphere.

At any rate it appears, that its height exceeded the sensible limits of our atmosphere; and that its light, and probably its heat, did not arise, as in our ordinary combustions, from the presence and decomposition of the oxigen gas of the atmosphere.

Probably stones fell from it.

Several circumstances of this phenomenon were similar to those that have been observed in lapidiferous meteors; and we should not be surprised to hear, that incandescent stones had fallen in places, which had this meteor in their zenith. No explosion was heard; but perhaps the distance was too great, and the circumambient medium too rare, for the sonorous vibrations to be transmitted to us.

XIV.

Letter from Professor P. PREVOST, *to Professor* PICTET *on the Meteor of the* 15*th of May**.

GENEVA, *May the* 28*th*, 1811.

Comparison of circumstances

THE care you have taken, my dear colleague, to determine exactly the position of the meteor observed the 15th of

* Bibliotheque Britannique, for May, 1811, p. 110.

of this month, will allow it to be compared with those ascertained by other observers. This is the only method of obtaining any accurate ideas respecting the vertical height, course, and nature, of these bodies foreign to our Earth, and the short passage of which cannot be foreseen.

of meteors desirable.

You have availed yourself of a favourable circumstance, to obtain the altitude and azimuth of that luminous object; but it is far from probable, that observers in other situations should be able to avail themselves of a similar proceeding: and should they report the height of the meteor, without having determined it by any instrument, we must expect great deviations.

Gresses at their heights and distances not to be depended on

There is a certain degree of confidence however, to be given to the estimations of men accustomed to appreciate their sensations, and compare quantities. If therefore such an observer should say, that he saw the meteor nearly at 45°, or at 30°, for example, this might be considered as probably coming pretty near the truth: because we may presume the observer, measuring in idea the interval from the zenith to the horizon, could pretty well estimate by the eye the half or third of that distance.

unless by experienced persons.

But there is a correction to be made in this estimate, which is scarcely thought of, but which, in loose observations of this sort, is in reality of great importance.

Necessary correction in the estimates of these,

The apparent firmament is a skene arch, which may be compared to an arc of a circle of about 60°: (see Smith's Optics, translated by Pezenas, vol. I, p. 117). If we construct a semicircle on a right line, and cut off an arch of about 60° to represent the apparent firmament, (as in the figure in Smith's Optics), we shall see, that half from the vertical of this apparent firmament answers to about 30° of real altitude, and a third to about 40°. Now it is easy to perceive the importance of such a correction, if we would obtain any accurate result from comparative observations, and in particular if we attempted to ascertain a parallax.

from the apparent firmament.

In confirmation of this remark I lay before you the observation of a man possessed of all the faculties calculated to mature his judgment in the estimation of measures. You will there see, that he estimates the height of the meteor between

Estimation by an observer at Geneva.

between 30° and 40°, that is between ¼ and ⅓ of the apparent distance from the zenith to the horizon. These points of division of the apparent firmament answer to 14° and 20°* of real altitude: and that of 18°, which you have measured, is between these two. I ought however to add, that the observer in question ultimately fixed on the estimate of a third, or 30° apparently. But all these determinations are necessarily approximations only.

XV.

Improvement in the Aquatinta Process, by which Pen, Pencil, and Chalk Drawings can be imitated: by Mr. J. HASSELL, *No. 11, Clement's Inn*†.

SIR,

Imitations of black lead drawings imperfect.

PERCEIVING the various methods of imitating drawings and sketches in the graphic art fall short of an accurate imitation of the black-lead pencil, I determined on an attempt, some years since, which, after repeated experiments, I flatter myself I have fully established.

The subject may be sketched with a pencil immediately on the copper.

The manner is totally new, and solely my own invention: —by the method I adopt any artist can sketch with a black-lead pencil his subject immediately on the copper, and so simple and easy is its style, that an artist can do it with five minutes study.

No retracing necessary.

By this manner, the trouble in tracing an oil paper, and other retracing on the etching ground is avoided, and the doubtful handling of an etching-needle is done away‡, as the

* A third of the arch of 60° from the horizon would give 20°, and $\frac{4}{9}$ 26° 40′. C.

† Trans. of the Soc. of Arts, vol. XXVIII, p. 97. The silver medal and thirty guineas were voted to Mr. Hassell for this communication.

Tracing rag.

‡ Tracing rag should be made of a piece of Irish linen, not too much worn, the surface of which is to be rubbed with another rag dipped in sweet oil, just sufficient to retain a small portion of vermilion or pounded red chalk. This must be placed with the coloured part towards the ground of the plate, and the drawing or tracing laid upon it, which must be traced very lightly with a blunt point or needle.

pencilling

pencilling on the copper is visible in the smallest touch:—It has also another perfection, that, by using a breader instrument it will represent black-chalk, a specimen of which I procured Mr. Munn, the landscape painter, to make a trial of. I have herewith sent the said specimen marked C, and Mr. Munn's name is affixed to the same. This subject he actually drew upon copper, under my inspection, in less than twenty minutes, the time he would have taken, perhaps, to do the same on paper; in fact, it can be as rapidly executed on copper as on paper. Black chalk. imitated.

It is particularly pleasant for colouring up, to imitate drawings, as the lines are soft, and blend in with the colour, It is a circumstance always objectionable in the common method of etching, that those so tinted can never be sufficiently drowned, nor destroyed, and always present a wiry hard effect. Particularly adapted for colouring:

It is equally adapted to historical sketching, and might be the means of inducing many of our eminent painters to hand down to posterity their sketches, which, at present, they decline, from the irksome trouble attending the repetition of retracing their performances, and the doubtful handling of the etching-needle, which can never give a sufficient breadth and scope to their abilities. and to preserve the sketches of painters.

I have, sir, forwarded, in an annexed paper, the different specimens, for the inspection of the gentlemen forming the Society of Arts, &c.

In making my specimens I have thought it necessary to show, that, if by any accident a part might fail, it could be retouched a second time, and oftener if wanted; in this particular its simplicity stamps its use. Any part capable of being retouched.

To elucidate the foregoing proposition, I purposely caused a part of the distance to fail in specimen A A; this is repaired you will perceive in specimen B, and the sharp touches wanted to perfect the sketch are added.

I beg also to state, it is not the style usually termed *soft ground etching*: that process is always uncertain, cannot be repaired, and will only print about two hundred impressions; whereas the specimens herewith sent will print upwards of five hundred, with care. Not soft ground etching.

Should the Society for the Encouragement of Arts &c. deem

...n the subject worthy of their reward, I shall feel proud in communicating its process, and flatter myself the arts and artists will feel a peculiar addition and pleasure in its utility.

Permit me, Sir,
to subscribe myself, with all respect,
Your obedient humble Servant,
JOHN HASSELL,
Landscape Draughtsman, 11, Clement's Inn, Strand.

Process of drawing upon Copper, to imitate Black-lead Pencil ... Chalk.

Method of drawing on copper ... tate bl... pe... ch...

A remark... must be put on the copper with an oil-rub... ... rtis well ground in oil; after ... with whiting, and then rubbed ...

... your plate the solution to cause ... ows;—

... es of Burgundy pitch.
... frankincense.

Preparation of the ground.

These are to be dissolved in a quart of the best rectified spirit of wine, of the strength to fire gunpowder when the spirit is lighted.

During the course of twenty-four hours this composition must be repeatedly shaken, until the whole appears dissolved; then filter it through blotting paper, and it will be fit to use*.

Application of it.

In pouring on this ground, an inclination must be given to the plate that the superfluous part of the composition may run off at the opposite side, then place a piece of blotting paper along this extremity, that it may suck up the

Grounds.

* The ground in hot weather must have an additional one third of spirit of wine added to it for coarse grounds, to represent chalk; and one half added to it for fine grounds, to represent black lead pencil; and always to be kept in a cold place in summer, and a moderately warm situation in winter.

N. B.—If any parts are not bitten strong enough, the same process is to be repeated.

ground

ground that will drain from the plate, and in the course of a quarter of an hour the spirit will evaporate, and leave a perfect ground, that will cover the surface of the copper, hard and dry enough to proceed with.

With an exceeding soft black-lead pencil sketch your design on this ground, and when finished take a pen and draw with the following composition, resembling ink: if you wish your outline to be thin and delicate, cause the pen you draw with to be made with a sharp point; if you intend to represent chalk-drawing, a very soft nib and broad-made pen will be necessary, or a small reed.

No. 2.—Composition, resembling ink, to draw the design on the copper.

Take about one ounce of treacle or sugar candy, add to this three burnt corks reduced by the fire to almost an impalpable powder, then add a small quantity of lamp-black to colour it; to these put some weak gum-water*, (made of gum-arabic), and grind the whole together on a stone with a muller: keep reducing this ink with gum-water until it flows with ease from the pen or reed. Ink for drawing on it.

To make the ink discharge freely from the pen, it must be scraped rather thin toward the end of the nib, on the back part of the quill, and if the liquid is thick reduce it with hot water.

Having made the drawing on the copper with this composition, you will dry it at the fire until it becomes hard: then varnish the plate all over with turpentine varnish†. Varnish.

It will now be necessary to let the varnish that is passed over the plate, dry, which will take three or four hours at least; but

* Gum water must be made in the proportion of half an ounce of gum arabic to a quarter of a pint of water.

† Turpentine varnish is composed of an ounce of black resin to an eighth part of a pint of spirit of turpentine; if the weather is excessively warm, it ought to be made with a sixth part of a pint of spirit of turpentine.

[I apprehend there is a mistake here, and that the proportions of spirit should be reversed; as more of the liquid would, no doubt, be required in cold weather than in hot. C.]

but this will depend on the state of the weather: for if it should be intensely hot, it ought to be left all night to harden.

Mode of rubbing off the touches.

Now the varnish is presumed to be sufficiently hard, you may rub off the touches made with the foregoing described ink with spittle, and use your finger to rub them up; should it not come off very freely, put your walling-wax round the margin of your plate, and then pour on the touches some warm water, but care must be taken it is not too hot.

The touches now being clean taken off, wash the plate well and clean from all impurities and sediment of the ink, with cold *soft* water; then dry the plate at a distance from the fire, or else in the sun; and when dry, pour on your aqua fortis, which should be in cold weather as follows:—

Acid.

To one pint of nitrous acid, or strong aqua fortis, add two parts, or twice its quantity of soft water.

In hot weather to one part of nitrous acid add three parts of water.

In every part of this process avoid hard or pump water.

Biting in.

The last process of biting in with aqua fortis must be closely attended to, brushing off all the bubbles that arise from the action of the aqua fortis on the copper.

In summer time it will take about twenty minutes to get a sufficient colour: in winter perhaps half an hour, or more. All this must depend on the state of the atmosphere and temperature of your room. If any parts require to be stopped out, do the same with turpentine varnish and lamp-black, and with a camel-hair brush pass over those parts you consider of sufficient depth; distances and objects receding from the sight of course ought not to be so deep as your fore-grounds; accordingly you will obliterate them with the foregoing varnish, and then let it dry, when you will apply the aqua fortis a second time, and repeat this just as often as you wish to procure different degrees of colour.

Stopping out.

Every time you take off the aqua fortis the plate must be washed twice with soft water, and then set to dry as before.

To ascertain the depth of the work.

To ascertain the depth for your work, you should rub a small part with a piece of rag dipped in turpentine, and then apply

apply the finger, or a piece of rag rubbed on the oil-rubber, to the place so cleared, and it will give you some idea of the depth.

The walling-wax is taken off by applying a piece of lighted paper to the back of the plate, all round the opposite part of the margin where the wax is placed; then let the plates cool, and the whole of the grounds &c. will easily come off by washing the plate with oil of turpentine, which must be used by passing a rag backwards and forwards, until the whole dissolves, it is then to be cleaned off by rags; and care must be taken, that no part of the turpentine is left hanging about the plate. Removal of the wax and varnish.

The plate should only pass once through the press. Printing.

SIR,

During the conference of the Committee of Polite Arts last Monday evening, an Essay on the Art of Aquatinting was produced, which, until that period, I had never seen; since then, I have procured a copy, and carefully perused it. As far as theory goes, respecting aquatinta, I allow it to be fair; but upon the practical part it is positively wrong, and what relates according to the opinion of your Committee as referring to my invention of the imitation of chalk and pencil-drawing, I can prove, by incontestible evidence, that I did produce specimens of my invention as far back as the year 1795 to the public, since which time I have improved the principle. The author's claim to the invention.

I flatter myself your goodness will enforce on the minds of those gentlemen who were present, that I ought personally to prove the same, which I am prepared with documents to do.

Permit me, Sir, to remark, after a lapse of fifteen years, that surely some person might have produced figures and landscapes sketched in this manner; but not a single artist, to my knowledge, ever gave one specimen to the public except myself, though my examples have been before them all the above time.

It is upon the application of the manner for freedom of imitating drawings, that I conceive it to be of importance, and

The author's claim to the invention.

and from this circumstance in pointing out its utility, I claim a credit from its originality. If, Sir, it was previously known, why was it not in use? The fact appears to me, that no person, except myself, thought of taking the pains to study the subject.

Having thus brought it publicly to notice, I still feel a degree of pride in furnishing an additional and easy step to the promotion of the arts.

I have now, Sir, to apoligize to you for trespassing on your patience, and as it is not possible for any gentleman to have taken more trouble, or have paid a more polite attention to the circumstance, I thought it most decorous to submit this memorial to you, as one of the Chairmen of the Committee of Polite Arts.

Trusting, Sir, you will be so good as to communicate the same to the Committee, I beg to subscribe myself, with all respect,

Sir,

Your very obedient humble Servant,

J. HASSELL.

No. 11, Clement's Inn, May 10, 1810.

To J. T. BARBER, Esq.

A Chairman of the Committee of Polite Arts.

XVI.

On the Nature of Oximuriatic Acid Gas, and the Conversion of Carbonic Oxide into Carbonic Acid by it, in Reply to Mr. J. DAVY. In a Letter from Mr. J. MURRAY, Lecturer on Chemistry, Edinburgh.

To MR. NICHOLSON.

SIR,

I Have not seen until lately Mr. J. Davy's communication in your Journal, for September last, and I embrace as early an opportunity as occurs to me, of offering a few observations in reply to it.

New gas sup-

The most important part of this communication is that, which

which relates to what he considers as a new gas, the operation of which, he supposes, serves to account for the production of carbonic acid, which I have found to be the result of the mutual action of oximuriatic acid, carbonic oxide, and hidrogen gasses. It is produced, he states, by exposing a mixture of equal volumes of carbonic oxide and oximuriatic gas to light, and he regards it as a compound of these two gasses.

posed to account for the production of carbonic acid.

I had already performed this experiment without obtaining the results he has described; and I am not aware of any fallacy, by which this can be accounted for; there was no sensible production of carbonic acid (the point I had it more particularly in view to ascertain by the experiment) and after agitation with water to remove the oximuriatic gas, the carbonic oxide was recognized by burning with its usual blue lambent flame, and forming carbonic acid by its combustion.

Not produced in an experiment of Mr. Murray's.

This result of the nonaction of oximuriatic gas on carbonic oxide gas, when both are perfectly dry, has been lately asserted still more strongly by Gay-Lussac and Thenard, and the terms they employ are even unusually decided. After observing, that the carburetted hidrogen gasses are acted on by oximuriatic acid gas when exposed to light, they add "mais à quelque dose qu'on ait mêlé le gaz acide muriatique oxigéné sec, et le gaz oxide de carbone préparé avec le fer et le carbonate de barite, quelque fort qu' ait été la lumière à laquelle on les a exposés, enfin quelque long qu'ait été le contact, il n'y a point eu d'action*". If I have been deceived therefore, it is in common with chemists of the highest reputation for the accuracy and delicacy of their experimental researches. These circumstances however lead me rather to believe, that there is some peculiarity necessary to the success of Mr. J. Davy's experiment. I know sufficiently the disadvantage to which any experimentalist

Oximuriatic and carbonic oxide gasses do notact on each other:

unless possibly under peculiar

* Recherches Physico-Chemiques, T. 2d, p. 192.

["But in whatever proportions we mixed dry oxigenized muriatic acid gas, and carbonic oxide gas procured by means of iron and carbonate of barytes, however strong the light to which they were exposed, and lastly however long they remained in contact, no action between them took place." C.]

circumstances.

mentalist is subjected, who undertakes the examination of experiments of which only a general account is given; and, from both these considerations, I am induced to suspend my experimental investigation of this subject until the more full account, which Mr. J. Davy announces he is to give of his experiments, is published. At present, I shall admit the production of this new gas, and shall offer merely a few observations on its relation to the present controversy.

Carbonic oxide converted into acid by oximuriatic gas,

In my first communication I had stated, that, when oximuriatic acid, carbonic oxide, and hidrogen gasses are submitted to mutual action, the carbonic oxide is converted almost intirely into carbonic acid. This result, inconsistent with Mr. Davy's hypothesis of the nature of muriatic and oximuriatic acids, was attempted to be explained by the assumption, that a portion of the water introduced to absorb the product of the action of the gasses had suffered decomposition, and that from this oxigen had been communicated to the carbonic oxide, so as to convert it into carbonic acid. Messrs. Davys, therefore, in repeating these experiments, employed ammonia to condense the product, and with this variation they found the carbonic oxide to remain unchanged.

alleged to be from the decomposition of water.

This disproved.

Though satisfied, that there was no probability in this assumption of water being decomposed, I thought it proper to repeat the experiment with the variation of condensing the product by ammonia. The result was still the same as that which I had before obtained. Nearly the whole of the carbonic oxide had disappeared, and a concrete salt was obtained, which effervesced strongly on the contact of a diluted acid, and also gave indications of the presence of carbonic acid by the test of muriate of barytes. I concluded therefore, as I believe any chemist would have done from these results, "that the production of carbonic acid in this experiment "was established beyond the possibility of doubt."

The same result obtained by Mr. J. Davy.

Precisely the same results have now been obtained by Mr. J. Davy. Repeating my experiment on the exposure of the mixture of the three gasses to light, he detected, "after the addition of ammonia, no traces of carbonic oxide": and he perceived, as I had stated, "an effervescence of the ammoniacal salt formed with nitric acid:" an effervescence which

which he farther admits to be owing to the disengagement of carbonic acid. The dispute therefore with regard to the fact is at an end; and the production of carbonic acid in these experiments, which I had always maintained to be the result, but which Messrs. Davys had denied, is established beyond the possibility of doubt.

Mr. J. Davy, however, forms a singular conclusion with regard to this. Having stated the results of his experiments, he adds: "after the preceding statement of facts, Mr. Murray, I should conceive, will be induced to renounce his conclusion, that the production of carbonic acid in his experiment was established beyond the possibility of doubt; and admit, that what he considered as carbonic acid was actually the new gas just described; and I should likewise imagine, that this gentleman, in future, will be more cautious in his assertions and criticisms on the labours of others." It is but justice to Mr. J. Davy to state on what grounds these expectations are founded. His conclusion

The result of the experiment with carbonic oxide, oximuriatic acid, and hidrogen gasses led him to repeat the experiment with the two former gasses alone. Having exposed therefore a mixture of carbonic oxide and oximuriatic acid without hidrogen to light, he obtained a similar result, a total condensation by ammonia without the slightest remains of carbonic oxide. By farther researches he found, that an acid gas is formed from the mutual action of the oximuriatic and carbonic oxide gasses, which combines with ammonia, and forms a concrete salt, and from the agency of this gas he explains the production of carbonic acid in my experiments. "I have now to announce", he remarks, "the existence of a new acid gas, which operated in Mr. Murray's experiments without his knowledge of its presence, and was the cause of those phenomena, which he erroneously attributed to the formation of carbonic acid gas." He supposes it to combine with the ammonia which is added, and to form a concrete salt; and "the decompo-"sition," he adds, "of this ammoniacal salt with effer-"vescence by dilute nitric acid deceived Mr. Murray." grounded on the supposed formation of a new acid.

On reading this paragraph I expected it to be proved, that no carbonic acid is disengaged from the concrete salt, and

and that the effervescence was found by Mr. J. Davy to be owing to the disengagement of this new acid gas. Then indeed, he would have had reason to say, I had been deceived; and grounds to form the expectation, that I should renounce my conclusion, that carbonic acid had been formed in my experiment; but in the succeeding sentence I found, sufficiently to my surprise, the admission, that it actually is carbonic acid, which is disengaged with effervescence, that my conclusion therefore is correct, and established by Mr. J. Davy's own experiments; and all that his labours amount to is, that by the aid of this gas he can frame an *hypothesis*, by which this production of carbonic acid, hitherto so steadily denied by him, may now, that he admits it, be accounted for in conformity to the opinion he defends.

The first question is the fact of the formation of carbonic acid,

It is obvious, that the first question in the controversy is with regard to the matter of fact. Is carbonic acid formed in these experiments, or not? How it is formed is a different question. I had uniformly maintained its production, or, that when carbonic oxide, oximuriatic acid, and hidrogen gasses are submitted to mutual action, the carbonic oxide disappears; and, whether the product be examined by the medium of water, or of ammonia, carbonic acid is obtained. Mr. J. Davy denied this. But it now appears from his own experiments, that my statement has been correct, that the carbonic oxide does disappear, and that carbonic acid is obtained. He therefore, I trust, will in future be more cautious in his assertions, and in calling in question the results of the experiments of others.

which is proved by Mr. J. Davy's own experiment.

His hypothesis to explain this.

The hypothesis, which he proposes to account for the facts now admitted, is the following. The carbonic oxide he supposes to unite with the oximuriatic acid, and form this new acid gas; it combines with the ammonia, and in the decomposition of this ammoniacal salt with effervescence, "water is decomposed, its hidrogen is abstracted by the oxi-"muriatic acid to form muriatic acid, and its oxigen by the "carbonic oxide to produce carbonic acid, which is disen-"gaged."

One would imagine from the manner in which the above sentence is expressed, that these were facts which had been experimentally

experimentally ascertained. They are however a series of suppositions, some of them in opposition even to the evidence, which Mr. J. Davy brings forward.

No proof that the new gas is formed in this experiment.

Thus no proof is given, that this new gas had been formed in the experiment. Admitting it to be formed when oximuriatic acid and carbonic oxide gasses are submitted to mutual action; it does not follow, that it will also be formed when they are in mixture with hidrogen. We know it is not formed when a little water is admitted, but that the products in this case are muriatic and carbonic acids. It is equally possible, that hidrogen may modify their mutual action so as to prevent its formation; that in this case also these acids are formed on the principle I have already explained; that the concrete salt formed with ammonia consists of muriate and carbonate of ammonia, and that the carbonic acid is directly disengaged from this salt by the diluted acid. There is not a single phenomenon attending the experiment as stated by Mr. J. Davy, which does not accord with this explanation.

Farther supposition, that this gas is capable of decomposing water:

It is farther an hypothesis, that this new gas is capable of decomposing water, when disengaged by an acid from its combination with ammonia; an hypothesis assumed to account for the production of carbonic acid, and supported by no proof. Mr. J. Davy says, indeed, that it must "appear evident, when it is known, that this new gas neither inflames on the passage of the electric spark with either oxigen or hidrogen alone, but that it detonates violently with a mixture of oxigen and hidrogen in proper proportions, and affords muriatic and carbonic acid gas." It is sufficiently evident, however, admitting even Mr. J. Davy's idea of the composition of this gas, that, when these gasses are in mixture with it, each of them exerting an affinity to one of its ingredients, without any affinity being exerted between them to counteract this, these combinations may be established; while it does not follow, that, when the oxigen and hidrogen are united by a strong affinity as they are in water, this will be overcome, and the water be decomposed. But why have recourse to these remote and indirect considerations? Let the fact be at once appealed to; does this gas decompose water or not? It appears from Mr.

the contrary of which appears. Mr. J. Davy's own account, that it does not; he states merely, that it is very slowly absorbed by water. It is therefore directly in the face of experimental evidence to assume, that, when it is disengaged from its combination with ammonia by an acid, it is capable of decomposing water; his hypothesis to account for the disengagement of carbonic acid falls to the ground, and the obvious conclusion must be admitted, that the carbonic acid has been formed by the mutual action of the carbonic oxide, oximuriatic acid, and hidrogen gasses, and that it exists in the concrete ammoniacal salt.

Mr. J. Davy will now perhaps perceive, that it was with some justice, that I maintained the fact of the production of carbonic acid in these experiments, and that I did not consider it invalidated by what was stated in opposition to it.

Mr. J. Davy's complaint of an expression of Mr. Murray's. He complains of an expression, which I employed in the discussion on this point, that "Messrs. Davys did not obtain carbonic acid in their experiments, because they did not look for it with sufficient care, or were not sufficiently aware of the sources of fallacy, by which its production might be concealed." It would be easy to justify this, not only from the results of my own experiments, in which carbonic acid was uniformly formed, results now proved by Mr. J. Davy's evidence to be correct; but from a review of the manner in which the results of the experiments to which I allude were examined. This I decline, however, as an invidious task, unless urged to it by Mr. J. Davy, referring rather to the brief observations, which I have occasionally offered on some of these experiments. Nor should I probably even have used this expression, had it not appeared to me called for by the tone, which has been assumed in this controversy, and the manner in which it has been conducted. If Mr. J. Davy will look back on its commencement, he will find, I believe, my first paper written with a degree of candour, to which it is not in his power to make a single objection. It was impossible, if an opinion were at all to be called in question, to have done so with more calmness and forbearance. Mr. J. Davy thought proper to take up the controversy in a very different spirit and style, and rendered it necessary for me sometimes to introduce a remark, which I should otherwise have avoided.

Of

Of the other parts of Mr. J. Davy's communication I may avoid, I believe, taking any notice. He has prefixed a kind of view of the progress of this discussion, in which are much repetition of what has been already replied to, and misstatements, which to those who have attended to the question it cannot be necessary to obviate. I shall merely give one example of this, and dismiss a subject sufficiently irksome. Mr. J. Davy has found, that, when a mixture of carbonic oxide, hidrogen, and oximuriatic gasses is inflamed by the electric spark, two measures out of ten of the carbonic oxide disappear; and this, he says, I "consider in my last communication as a demonstration, that oximuriatic gas is a compound of an unknown basis and oxigen". There is not a sentence in that communication of mine, that will fairly admit of such an interpretation, nor should I have thought of resting any demonstration on so narrow a basis. I considered the fact established by my own experiments, that there is a total or nearly a total conversion of carbonic oxide into carbonic acid, as such a demonstration. I have farther considered this partial conversion of carbonic oxide into carbonic acid in Mr. J. Davy's experiment, as *a confirmation to a certain extent of my results*; and I pointed out to him a very sufficient reason, why its success had not been more complete--his having diminished the proportion of hidrogen to less than one half of that which I had employed. It is not more necessary perhaps to take notice of his remarks with regard to the action of oximuriatic gas on carburetted hidrogen. He must have known of the difference of opinion, which prevails among chemists with regard to the carburetted hidrogen gasses, and of course, in giving an account of any experiments upon them, he ought to have mentioned what particular gas he employed. The gas from humid charcoal has been regarded as a variety of carburetted hidrogen, it is the one even to which the name was first given, and to which it is still applied; and though different opinions exist with regard to its constitution, I could not know what opinion Mr. J. Davy held with regard to it, or what he considered as exclusively carburetted hidrogen. The subject however is one of little importance, and my observations with regard to the one gas will

Instance of a misstatement by Mr. J. Davy.

Mr. J. Davy does not say what kind of carburetted hidrogen he used.

will still, I believe, hold just with regard to the other, nor would there be any difficulty in showing the imperfections of Mr. J. Davy's experiment.

Mr. Davy's explanation of phenomena hypothetical and unproved.

The question with regard to the general merits of the subject, I conceive now to be at rest. Mr. Davy's opinion, which was first held out as a genuine theory, admitting of no doubt as being a simple expression of facts, has been shown to be a hypothetical explanation of phenomena. And as an hypothesis not a single proof has been given of its truth, or no fact has been brought forward, exclusively explained by it, or explained with more probability than by the opposite hypothesis. It requires in its adaptation to the phenomena more multiplied and complicated assumptions, and it is at variance with the most strict and extensive analogies.

Mr. Murray's opinion agreeable to that of Berthollet, Gay-Lussac, and Thenard.

I am pleased to find my opinion on this point sanctioned by that of Berthollet, and of Gay-Lussac and Thenard. That by the latter chemists is of too great a length to permit me to introduce the quotation. I therefore refer to their memoir*. Berthollet, in a report on their researches, has given a more condensed view, equally clear and candid, his opinion cannot be received without interest by chemists, and you may therefore perhaps find room for the insertion of it. After remarking, that Gay-Lussac and Thenard had concluded, from their experiments, that oximuriatic acid gas may be a simple substance, and that all the phenomena it exhibits may be explained on that hypothesis, but that they had preferred the common hypothesis, as explaining them still better, a preference they continue to give notwithstanding the other idea has been adopted by Mr. Davy; Berthollet adds,

Berthollet's remarks on Mr Davy's hypothesis;

"In fact, to consider the oxigenized muriatic gas as a simple substance, we must suppose, that common muriatic acid is a compound of hidrogen and oxigenized muriatic acid; and that the metallic muriates are of a nature entirely different not only from other metallic salts, from these very muriates themselves dissolved in water. We must suppose, that lime and magnesia give out oxigen, the existence of which in them is supported by certain experiments, according to another hypothesis, to combine in the

* Recherches Physico-chimiques, T. 2d, p. 165.

metallic

metallic state with oxigenized muriatic gas; and that this gas combines with the oxigen, which the water gives up to it, to pass to the hyperoxigenized state: and these suppositions are not sufficient to explain every thing.

"In the other hypothesis, that is to say, admitting that oxigen is capable of combining with muriatic acid, as it is with metals, and with all combustible substances, all the explanations are natural, and perfectly analogous with those given of other facts, in which oxigen is transferred from one substance to another. Only the new observations show, that, to effect the change of oxigenized muriatic gas into muriatic gas, it is necessary for the latter to be in a situation to receive the quantity of water necessary to its constitution; which agrees with the force of its combination, which is very great in muriatic acid. on the former hypothesis;

"It may not be useless to remark, that, when we discuss the nature of substances, the mode of their combination, and the changes that may take place in the elements that enter into their composition, it is easy to multiply hypotheses: but those that are best supported by analogy, and require the fewest suppositions to connect them with the facts, so that the mind readily embraces their relation to them, should be adopted; still however not confounding their applications with the facts themselves confirmed by weight and measure, or with the inductions that immediately flow from these*." and on hypotheses in general.

A few months ago I commenced a train of experimental investigation, different from that which I have hitherto prosecuted, which promised to be decisive with regard to these hypotheses. The results of the experiments I have performed have accordingly been such as appear to me to establish the truth of the common opinion. An account of these will, with your permission, form a communication for the succeeding number of your Journal. New experiments promised.

I am, with much respect,

Edinburgh, 17*th Oct.* 1811.

Your most obedient servant,

JOHN MURRAY.

* The quotation in Mr. Murray's paper was in the original French: but for the sake of those of our readers, to whom that language is not sufficiently familiar, it is here given in English. C.

XVII.

XVII.

METEOROLOGICAL JOURNAL.

	Wind	Pressure. Max.	Min.	Med.	Temperature. Max.	Min.	Med.	Evap.	Rain	
9th Mo.										
Sept. 9	E	30·17	30·15	30·160	75	51	63·0	—		☽
10	E	30·15	30·05	30·100	77	46	61·5	—		
11	Var.	30·15	30·00	30·075	80	55	67·5	·45		
12	E	30·19	30·15	30·170	73	53	63·0	—		
13	E	30·11	30·01	30·060	71	40	55·5	—		
14	E	30·02	29·98	30·000	74	45	59·5	·56		
15	N E	30·07	30·02	30·045	70	55	62·5	—		
16	E	30·05	29·98	30·015	66	44	55·0	—		
17	E	30·05	29·95	30·000	70	47	58·5	—		●
18	E	29·95	29·87	29·910	71	44	57·5	·95		
19	S E	29·87	29·50	29·635	74	47	60·5	—		
20	S E	29·53	29·50	29·515	74	51	62·5	—	—	
21	S	29·80	29·53	29·665	66	54	60·0	·32	·08	
22	Var.	29·80	29·60	29·700	65	52	58·5	—	—	
23	S W	29·60	29·43	29·515	61	43	52·0	—	·14	
24	N W	29·62	28·92	29·270	64	49	56·5	·30	·14	
25	W	29·26	28·86	29·060	60	48	54·0	·10	·46	☾
26	W	29·21	29·17	29·190	61	39	50·0	—	·10	
27	S W	29·33	29·20	29·265	51	40	45·5	·18	·45	
28	N W	29·54	29·33	29·435	62	44	53·0	—	—	
29	N W	29·71	29·54	29·625	64	51	57·5	—	·20	
30	S W	29·72	29·55	29·635	62	50	56·0	·22	·16	
10th Mo.										
Oct. 1	W	29·76	29·47	29·615	63	46	54·5	—	·04	
2	S W	29·87	29·85	29·860	64	39	51·5	·19		○
3	E	29·85	29·46	29·655	61	52	56·5	—	·26	
4	S	29·57	29·46	29·515	69	59	64·0	—	·02	
5	S W	29·75	29·57	20·660	67	54	60·5	·35	·15	
6	S W	29·95	29·87	29·910	63	52	57·5	—	·18	
7	S W	29·92	29·85	29·885	69	54	61·5	·21	·01	
8	S W	30·00	29·90	29·950	67	53	60·0	·10		
		30·19	28·86	29·736	80	39	57·85	3·93	2·39	

N. B. The observations in each line of the Table apply to a period of twenty-four hours, beginning at 9 A. M. on the day indicated in the first column. A dash denotes, that the result is included in the next following observation.

NOTES.

NOTES.

Ninth Month, 9. Before sunset, after a serene day, *cirrus* clouds, pointing downward, from the W. 11. *Cirro, cirrocumulus*, some dry haze: wind westerly by night, scarce sensible. 14. *Cirri* and haze in the evening twilight of a bright orange colour. 15. Much wind: clear. 16. a. m. overcast: p. m. clear: twilight duller, with *cirrostratus*. 17. Much wind: very clear sky. 18. As yesterday: evening twilight luminous, orange, surmounted with rose colour, the latter somewhat in converging streaks. 19. Morning twilight obscure, with dense *cirri*: much dew: wind, a. m. N. E. Thunder clouds at different heights, some of which moved from the S. E. There were clouds throughout the night, with lightning. 20. Wind a. m. N. E. Thunder clouds again, which grouped, and passed about 2 p. m. to the W. with a few drops: *nimbi*, with a faint bow in the distance: evening cloudy, with two strata: wind S. E.: much lightning in the S. W. 21. a. m. Cloudy. Rain, with distant thunder at one and two p. m.: *Nimbi* and *cumulostratus*: faint bow. 22. a. m. Overcast. Wind veered to N. W., apparently by E. *Cirri*, in lines from N. E. to S. W. 23. a. m. Wind fresh from S. W., with rain: p. m. fair, with various modifications of cloud, which were finely coloured at sunset in the east. 24. a. m. Clear: much dew: fair day, but with clouds indicating rain: twilight milky, with a blush of red: the moon disappeared early, behind *cirrostratus* clouds, and it rained heavily in the night. 25. Cloudy and windy, with rain. 26. a. m. *Cirrus* with *cumulus*: p. m. showers. 27. Windy: wet. 28. a. m. Misty: p. m. showers, *cirrostratus*, and a blush on the twilight. 29. Evening, lightning: wet night. 30. Lunar halo.

Tenth month, 1. a. m. Wind S. E. showery. 2. A little before sunrise I observed a *stratus* in the marshes to the S. E., very nearly resembling a sheet of water; one which was seen from this village, in similar circumstances, about two weeks since, was actually taken by several persons for an extensive inundation. In the afternoon, large elevated *cirri* and *cirrostrati* rapidly passing at sunset from red to gray, indicated a renewal of the wet weather. 3. Misty morning, with *cirrostratus* above: very wet, p. m. 4. Much wind: cloudy night. 5. Squally. 6. a. m. Cloudy, much wind: evening calm; large *cirri* and *cirrostrati*, with a blush on the twilight: a bright blue meteor in the N. W.: wet night. 7. Cloudy, with a gale of wind. 8. Fair.

RESULTS.

Barometer: highest observation 30·19 inches; lowest 28·86 inches; range 1·33 inches.
Mean of the period 29·736 inches.
Thermometer: highest observation 80°; lowest 39°; range 41°.
Mean of the period 57·85°.
Evaporation 3·93 Inches. Rain 2·39 inches.

From the full moon of last period to the new moon of the present, easterly breezes with clear days, and the *stratus* by night. Evaporation went on increasing as the wind became stronger: dew fell in plenty, and the small meteors, called shooting stars, were abundant. The latter half of the present period brought the accustomed compensation, in rain from the westward: the approach of this was perceptible for several days beforehand; and the ground being dry, it was attended at the beginning with some discharges of electricity from the clouds.

Several persons, imagining they perceived something extraordinary in the weather, have enquired, whether the present *comet* could have any influence upon the seasons. It would be idle to reason upon its power without proof of its effects; and these, again, must be proved to extend, at least, over the whole northern hemisphere; for which a corner of our little island is no adequate standard. It seems within the limits of possible conjecture to say, that comets *may* induce some change in the atmosphere of the planets, by changing the state of the æther (if there be any such medium,) interposed between these and the sun; or by affecting the production of luminous matter on the surface of the sun itself. A comet approaching near to a planet would also disturb the atmosphere of the latter by the mere effect of its attraction: but we have a planet attendant on the Earth, which is doing this every day, and we are still unprepared duly to appreciate its power. Comets are, therefore, at present, out of the province of the meteorologist.

L. HOWARD.

PLAISTOW, *Tenth Mo.* 16, 1811.

XVIII.

Experiments on the acid Phosphate of Potash: by Mr. VAUQUELIN*.

Crystallizable compound of phosphoric acid and potash.

MR. Vitalis, secretary to the academy of sciences, letters, and arts at Rouen, and professor of chemistry in that city, having formed, in the course of his operations, a compound of phosphoric acid and potash, each extremely pure; and having obtained, by suitable evaporation, a perfectly crystallized salt; presumed that other chemists, who have all announced the uncrystallizability of phosphate of potash, were deceived.

Too modest to take on himself to contradict what had been said on this head by the ablest chemists, he sent me a small quantity of the salt, that I might examine it, and give him my opinion of it. The following are the results of my researches.

Its properties.

1. This salt is very white, crystallized in prisms with four equal sides, and terminated by pyramids with four faces corresponding to the sides of the prism.

2. It has a very sour taste, and powerfully reddens infusion of litmus. It is not alterable by the air.

3. With lime-water it throws down a copious, white, flocculent, and as it were gelatinous precipitate.

4. Caustic potash evolves from it no ammonia.

5. It forms a copious precipitate with solution of muriate of platina.

6. It gives out no phosphorus by the action of heat, but it melts into a clear glass, which crystallizes and becomes opake on cooling.

7. After having been thus melted, it does not dissolve in water so easily as before.

Uncrystallizable when neutralized.

8. A portion of this salt having been saturated with potash, and subjected to spontaneous evaporation, did not crystallize: it was reduced to a kind of viscous liquor, resembling a solution of gum.

A superphosphate of potash.

From these experiments it evidently follows, that the salt in question is an acid phosphate of potash; consequently,

* Ann. de Chim. vol. LXXIV, April, 1810, p. 96.

that

that what chemists have said of the common phosphate of potash is not affected by the properties it exhibits: and that Mr. Vitalis has enriched chemistry with a new species of salt, to be placed in the class, already very numerous, of these substances.

SCIENTIFIC NEWS.

A Card has been transmitted to the subscribers to the Scientific Institution, Princes' street, Cavendish square, announcing the commencement of the annual Lectures at that Establishment, on Tuesday the 19th of November. Lectures at the Scientific Institution.

The arrangement embraces the following subjects:

A popular course of twelve Lectures, on the most interesting branches of Experimental Science, by Mr. George Singer: a course on the Philosophy of the Mechanic Arts, by Mr. E. Lydiatt: and a course of twelve Lectures on Chemistry, by Mr. George Singer.

Surry Institution, Blackfriars Bridge.

The annual courses of Lectures at this Institution will be delivered in the following order, viz. Lectures on natural philosophy, chemistry, music, and belles lettres.

1. On the Philosophy of Physics, by I. M. Good, Esq. F. R. S., Mem. Am. Phil. S., and F. L. S. of Philadelphia; to commence on Friday, Nov. 22, and be continued on each succeeding Friday.

2. On the Belles Lettres, by Edward Quin, Esq, to commence on Tuesday the 26th Nov., and be continued on each succeeding Tuesday.

3. On the Chemical Phenomena of Nature and Art, by Fred. Accum, Esq., M. R. I. A., F. L. S.; to commence early in 1812.

4. On Music, by W. Crotch, Mus. D., Professor of Music in the University of Oxford; to commence early in 1812.

Mr.

Lectures on manufactures.

Mr. Clennel, Conductor of the "new Agricultural and Commercial Magazine, or General Depository of Arts, Manufactures, and Commerce", commences a course of six weekly Lectures on Manufactures, at Stratford, near Bow, on the 1st of November. Iron, coal, wool, cotton, linen, and silk, with the various arts and manufactures arising out of or connected with them, will form the leading topics of these discourses, which are intended to be amusing, as well as instructive.

Mathematical papers in the Ladies' Diary.

Mr. T. Leybourn, of the Royal Military College, Editor of the Mathematical Repository, intends to publish by subscription a Collection of all the Mathematical Questions, and their Answers, which have appeared in the Almanack called the Ladies' Diary, from its commencement in 1704 to the present time. The editor of the Diary (Dr. Charles Hutton) published a similar work in 1773, but comprehending both its mathematical and poetical parts down to that period. Mr. Leyburn's publication will comprehend only the mathematical part, and, with Dr. Hutton's permission, will contain all the valuable additions given in his edition, as far as it extends. He also hopes to be able to give other additions by the assistance of some of the ingenious mathematicians, who have for a number of years past contributed to the Mathematical Repository.

The work will be printed in 8vo, and will be published in half volumes, one of which will appear every three months. The diagrams will be printed in the text, from figures cut in wood. It will be put to press as soon as such a number of subscribers can be obtained, as shall give the editor a prospect of being indemnified for the expense, which must attend its publication.

Parkinson's Organic Remains.

The 3d vol. of Mr. Parkinson's Organic Remains of a former World is promised in the course of November.

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

DECEMBER, 1811.

ARTICLE I.

Description of a Spire of a new Construction, at Edgeworthstown, combining the Advantages of Cheapness, Elegance, and Durability. In a Letter from RICHARD LOVELL EDGEWORTH, *Esq., F.R.S. M.R.I.A. &c.*

To W. NICHOLSON, Esq.

SIR, EDGEWORTHSTOWN, IRELAND, *Sept. the* 22*nd*, 1811.

I Have lately erected a spire of a new construction on the tower of the church of Edgeworthstown, and, as the attempt has succeeded, I hope an account of it will be acceptable to your readers. Spire of a new construction at Edgeworthstown.

My object was to lessen the expense, and to facilitate the means of ornamenting places of public worship.

This spire is fifty feet high from the base to the star by which it is crowned. See Plate VII, fig. 1, which is a section of the tower, the spire, and part of the machinery. The spire was made withinside of the tower, and, when completely finished, was drawn up in a few minutes by machinery, and placed on the tower, where it now stands. It consists of a skeleton of hammered English iron, covered with Height of the spire. Mode of its construction.

with strong Welsh slates, capped where they meet on the skeleton by large copper beading, which, with the slates, is fastened to the skeleton by copper bands and cramps. The whole is well painted, and covered with sand, so as to imitate stone.

The skeleton.

The skeleton was formed of eight bars of iron, 45 feet long, 2 inches and ½ broad, and ¾ of an inch thick. These dimensions were chosen because they are those of common bars, that are sold by ironmongers. These bars are usually 14 or fifteen feet long, and I had them welded in a common forge to the length that was requisite. Eight of these were disposed octagonally upon a base, fig. 3, about 9 feet in diameter, which is nearly the diameter of the tower. It was made of bar iron an inch square.

Manner in which the parts were fitted together.

Before the spire was put together in the tower, the parts were previously fitted on the ground, not perpendicularly, but lying sideways, so that each bar could be easily reached by the workmen. With this view I took advantage of a saw-pit, which permitted half the base to lie below the ground, while the apex, or point of the spire, was supported by a bench, on the surface of the ground. This enabled me to assemble and fit the bars which were necessary for cross braces, and to combine the bars accurately round the spindle of the weathercock, and to secure them by a ring of iron.

The base.

The base above mentioned, fig. 3, consisted of four bars of iron, flattened where they crossed each other, with a hole through the middle of each, that received a bolt to bind them together. The ends of each of these bars were so formed, with cheeks, as to permit the bars, that composed the spire, to lodge within them, and to be fastened to them by screw bolts. Light flat bars *d*, *d*, *d*, held by the same screw bolts, were placed between the bars of the spire, to keep them at due distances from each other, thus forming a species of *diaphragm*, fig. 3, where A represents the diaphragm resembling the rudiments of a spider's web, *c c c*, &c. the cheeks of each transverse bar of the diaphragm, and *b b b* the bolts, which connect them with the legs of the spire.

Angular braces.

Beside these diametrical supports there are four bars, B B,

B B, fig. 6, 20 feet long, placed obliquely from the bottom of one bar to the opposite bar, to which they are connected by screw bolts, thus forming angular braces.

Spindle. The spindle of the weathercock rises 5 feet above the apex of the spire, and, passing downward through the junction of the bars, it is inserted into a solid diaphragm under and against which it is keyed by a forelock.

Diaphragms. Beside this diaphragm, and that which forms the base of the spire, there are three others D D D, fig. 6, of a construction similar to that of the lowest diaphragm, placed at equal distances from each other. It is to be observed, that the cheeks or ends of the three upper diaphragms project beyond the upright legs of the spire, to assist in supporting the slates; but the cheeks of the lower diaphragm take in not more than two inches of the feet of the bars of the spire; which feet, as may be seen at fig. 4, are considerably broader than the rest of the bars.

Feet of the bars. At B, fig. 4, a tenon is formed at the heel of the foot of each bar, which is to receive a key, or forelock, to fasten the spire to the tower, after it has been raised to its place.

Carriage for raising the spire. To raise and guide this spire, a pedestal, the plan and section of which are seen at fig. 2, and 6, was constructed. It consists of a top and base, each formed of four pieces of deal 6 inches square, and of eight jambs, or uprights, of the same breadth and thickness, and 10 feet high, morticed into the base and top, so as to stand nearly under the eight legs of the spire when it is raised upon it. See fig. 6, where J J J show the position of these uprights. The uprights are strengthened by braces, *o b b o*, so as to prevent them from *racking*, or moving obliquely. The pedestal was furnished with eight wheels 6 inches in diameter, at its upper corners; and with eight similar wheels at its lower corners; as in the plan, fig. 2, and in the section, fig. 6, *w w*.

To facilitate and guide the movement of this pedestal upwards, the tower was lined at each corner with thin planks, P P, fig. 1, fastened to the walls perpendicularly, and adjusted with care. Against these planks the wheels of the pedestal moved upwards with little friction, keeping the spire perpendicular in its ascent.

When this pedestal was adjusted, the skeleton, which had been fitted on the ground, was taken to pieces.

Manner in which the spire was put together.

The base, or lower diaphragm, upon which the bars had been adjusted, was placed and fastened in a temporary manner on the pedestal. The long bars were drawn up, one by one, into the tower above the platform; and their feet were inserted into the cheeks of the base, or lower diaphragm; where they were secured by bolts, as before described. The other diaphragms, and the iron cross braces, were then inserted between the iron bars, and firmly bolted to them.

Covered with slates.

By the favour of Messrs. Worthington and Co. of Penrhyn, I was furnished with excellent slates of dimensions sufficiently large to cover the spaces between the bars, which at the base were nearly 4 feet wide. The slates were 2 feet 6 inches high, and nearly an inch thick*. These slates were sawed to fit upon the ribs where they met, and they were rabbeted with the saw and chissel to lap over each other, so as to keep out water. They were so well joined by these means as to present one even surface, on which the courses of the slates scarcely appeared through the paint. These joints might by addional paint have been entirely concealed, but their appearance was thought to be advantageous, as it gave an idea of solidity, from its nearer resemblance to stone.

Mode of fastening the slates.

It remains to show how the slates were fastened to the iron upon which they were placed. For this purpose grooves about one quarter of an inch deep were sawed in the upper surface of each slate, parallel to the bars, and at the distance of nearly two inches from them. A copper capping,

Best saws for cutting slates.

* The slates were first cut with sand, and such saws as are used for cutting marble. Though this is the method followed at Penrhyn, I found common saws of a smaller size, such as are usually sold for half a crown, far more expeditious.

In cutting the grooves, that receive the copper capping, I employed thin saws with a wooden back, which was held in the hand of the workmen. To make these saws, I cut the blade of small saws into four parts with common tinkers'sheers.

Air holes cut in them.

Air holes in form of a *quatre feuille* were made near the top of the spire, to permit the circulation of air, and they serve also to facilitate the application of a moveable scaffold, whenever the spire requires new painting.

early

nearly semicircular, and about four inches in diameter, was placed so as to cover the joints of the slates, where they met the bars, sinking into the grooves which were just sufficiently wide to receive the copper. The copper by its shape and elasticity *caught* in the grooves, so as to form, when painted, a covering perfectly impervious to rain and snow.

Mode of fastening the slates.

To fasten these copper caps and the slates to the skeleton of the spire, a contrivance was adopted, which requires some detail to become intelligible. The general idea was to fasten the capping and the slates *from within*, so as to leave no holes to be stopped on the outside by putty or paint. Fig. 7 is a section of the slates on a larger scale than that of the spire, where they join on the rib; of the copper capping; and of a collar, or band, by which they are connected with an iron cramp, that passes round the inside of the rib, and, hooking into the collar or band, is wedged within, against the inside of the rib. In looking at this section, care must be taken to distinguish the circular edge of the copper capping from the edge of the band or collar. The band, as may be seen in the drawing, is twice as thick as the capping. In this section of all these parts, as connected together, C is the copper capping; S S, the band or collar; H H, the cramp, or holdfast; and W, the wedge.

The whole of this apparatus for fastening the slates succeeded to my wishes: it was easily executed by common workmen; the parts were easily put together; and, when adapted to their several places, they held the slates and their capping firmly upon the bars, at the same time producing a very good effect by raising a bold and ornamental moulding, or *torus*, fig. 7, on every angle of the spire. It is scarcely necessary to add, that part of the lower corner of each slate was cut away at A to permit the cramps to pass through, and to embrace the iron rib; and that the ends of the diaphragms were permitted to extend beyond the outward surface of the ribs, to support the perpendicular pressure of the slates. Such slates as were not thus supported rested upon the rabbets of those that were beneath them.

The machinery, by which the spire, when it was thus finished, was drawn up, must now be described.

Description of the machinery for raising the spire.

The

The pedestal. The plan of the pedestal, the top and bottom of which are similar, is represented at fig. 2, where 1, 2, 3, 4, &c. are the bottoms of the eight jambs, or uprights, of the pedestal; and W W, &c. the wheels, or rollers.

A section of the pedestal, fig. 6, is drawn in the inside of the section of the tower.

b b, cross braces.

The spire. D D the base, or lower diaphragm, of the spire, resting on the pedestal, to which it is attached by four bolts (of which two only are seen in the section) with forelocks, F F, so as to be easily detached from each other.

L L L L, the legs of the spire.

D D, the diaphragms.

S, the spindle of the weathercock, passing through the apex of the spire.

C, a conical collar, or ring, enclosing the top of the legs. A shoulder is formed on the spindle, and rests on this ring; and as the collar, or ring, projects a little above the tops of the legs of the spire, it could be forced downwards, till the shoulder touches the tops of all the legs, which are cut even, and horizontal at top, so as to permit the collar, the legs of the spire, and the spindle, to be firmly bound together. This is done by means of a mortice, or keyhole, formed in the lower part of the spindle which passes through the small solid diaphragm *d*, against which it is wedged by the forelock *f*.

Method of fastening the spire in its place on the tower. The heels of all the bars, with the tenon at B, fig. 4, (where it is drawn upon a larger scale) pass through consols, X X, fig. 1, of stone capped with cast iron, that project from the wall of the tower. The iron cappings of these consols, fig. 8, are made of cast iron, and have apertures left in them, through which the heels of the bars, which form the spire, may pass. When they have all been raised through the consols, eight washers, fig. 9, with a mortice, *m*, in the centre of each of them, are laid upon the consols, and, the spire being allowed to descend, the tenons in the heels of the bars fall into the mortices, and rest upon the consols, and eight other washers are placed upon the tenons, under the consols beneath which they are keyed by forelocks.

T T, fig. 1, the walls of the tower.

W W,

W W, the horizontal windlasses, over which two of the ropes were coiled, once round, with weights hung to them.

r r, pullies, over which the ropes passed. Of these there were ten sets, with weights, to counterpoise the pedestal and spire.

h h, handspikes.

Four men were sufficient to work both the windlasses; and on the 19th of this month, before a very respectable concourse of spectators, the spire was drawn up without difficulty or noise in eighteen minutes. It was soon detached from its pedestal, and fixed in its proper place on the consols, with the washers and keys, or forelocks. The spire drawn up,

A sufficient number of the counterbalancing weights were cut off by sheers; and the men, who had worked the windlasses, descended upon the pedestal to the bottom of the tower.

A plumbline was hung from the top of the spire within side, by which it was properly adjusted; and by a few wedges it was placed perfectly upright. placed truly perpendicular,

To add security to the connexion between the spire and the tower, iron cramps of 7 or 8 feet long were hooked into the mortices, which had served to join the legs of the spire to the pedestal, and were firmly fastened to the walls of the tower by proper holdfasts: so that, though the spire and tower may be blown down together, it is scarcely possible, that they can be severed by the violence of any storm. and farther secured.

The cost of this spire has not yet been entirely ascertained, but it does not exceed one hundred and fifty guineas. A spire of the same dimensions, built of Portland stone, would, in this country, cost at least six times this sum, and if it were formed of the limestone of the country, it would cost four or five hundred pounds. Expense of the spire.

I was this day, September the 22nd, enabled to determine, whether strong wind had any sensible effect on the spire, as its spindle happens to coincide with a vertical wire of a transit instrument in my observatory. The violence of a sudden squall did not seem in the least to affect it.

I have therefore reason to hope, that it will remain undisturbed by future storms: and, as a thunderstorm passed over this place the night before, I trust, that the conductor, which

which has been attached to the iron legs, will secure the spire from the effects of lightning.

I am, Sir,
Your obedient servant,
RICHARD LOVELL EDGEWORTH.

It has occurred to me since the spire was finished, that, instead of a temporary wooden pedestal, an iron permanent pedestal might be substituted, which might be formed by a continuation of the legs of the spire. At the base of this pedestal, if it were thought necessary, a brick arch might be turned on the lowest diaphragm. This would add weight, and consequently solidity to the mass. This pedestal must be connected with the tower by holdfasts and wedges.

I mention this, not because I find any inconvenience in what I have executed, but to communicate to the public all that has occurred to me on this subject.

II.

Experiments on some Preparations of Gold: by Mr. VAUQUELIN*.

Preparations of gold employed medicinally.

SINCE Dr. Chrestien, of Montpellier, mentioned the effects he had obtained from the use of preparations of gold in syphilitic and lymphatic complaints; and remarked, that these effects were never attended with the ill consequences, to which mercurial preparations often give rise, other physicians have begun to make use of them.

The forms in which gold has hitherto been employed are, 1, in a state of minute division: 2, the muriate: 3, the oxide precipitated from a solution of gold by potash: 4, the precipitate thrown down by metallic tin from the muriatic solution of gold.

* Annal. de Chim. vol. LXXVII, p. 321.

There

There is some difficulty in obtaining these preparations constantly in the same state; and one of the principal objects in the art of physic being precisely this constancy in the nature of medicines, it appeared to me of some utility to examine these preparations, and to describe with accuracy the processes best adapted for obtaining them. Difficult to obtain these uniform.

Sect. 1. *Of the quality and quantity of nitromuriatic acid most suitable for dissolving Gold.*

It was formerly the practice, to compose nitromuriatic acid of two parts of nitric and one of muriatic, by weight. But on considering, that gold requires only a very small portion of oxigen for its solution, and that the nitric acid in the process in question answers this purpose alone, I concluded, that the same purpose would be obtained, if an aqua regia were composed of the two acids in opposite proportions to those hitherto directed. In fact, three parts of nitromuriatic acid thus made were sufficient to dissolve one part of fine gold, while at least four made in the old way were required, Nitromuriatic acid. 2 p. muriatic, 1 p. nitric, dissolve 1 p. of gold.

A proof of the small quantity of oxigen, that combines with gold at the moment of its solution may be found in the very small quantity of nitrous gas evolved: beside which there is reason to presume, that some portion of this gas is produced by the action that takes place between the two acids, since some oximuriatic acid is evolved likewise But little oxigen combines with gold.

The solution of gold, when duly evaporated, crystallizes in yellow prisms, the figure of which, I believe, has never yet been ascertained with precision. The solution crystallizes,

The evaporation of the solution must be conducted with great caution, otherwise part of the salt will be decomposed, and the gold will reappear in its natural state, in the form of small scales. but is partly decomposed without great care.

The solution of muriate of gold comports itself with the fixed alkalis in a manner different from that of other metals; most of which, it is well known, are completely precipitated by them in the state of oxide. Action of alkalis on it.

Potash, soda, barytes, and lime, do not render the solution of gold in the least turbid, at common temperatures. It only acquires a very deep red colour with potash and soda Do not precipitate it,

soda, nearly like that of Stahl's alkaline martial tincture. No change in the limpidity of these mixtures takes place on standing.

Barytes and lime do not produce the same colour in the solution of gold, no doubt on account of the great quantity of water employed in their solution.

unless assisted by heat.

If, after the acid of the solution of gold has been completely saturated by potash, the mixture be heated, a red substance separates in a very bulky flocculent form, much resembling in appearance oxide of iron at a maximum.

Precipitate with excess of alkali.

If an excess of caustic alkali, even though very trifling, be put into the mixture, and it be boiled, the bulk of the precipitate will diminish greatly, and it will appear of a brown colour, when seen in a body; though it is in reality blue, for the particles of matter suspended in the liquor, *which of itself is slightly yellow, make it appear green**.

The menstruum still retained some gold.

The liquid, from which I had precipitated the matter abovementioned by means of potash, was colourless; but, as soon as it was saturated with muriatic acid, it suddenly assumed a yellow hue, like that of the common solution of gold, and sulphate of iron threw down metallic gold from it.

The precipitate slightly soluble.

All the washings of the precipitate, to the last, gave signs of the presence of gold; which seems to indicate, that this matter is slightly soluble in water. The last washings however contained less than the first.

Action of sulphate of iron on solutions in different proportions.

When the liquors contain a certain quantity of gold, the precipitate formed in them by sulphate of iron presently assumed a brown colour; but when they contain only a little of this metal, no precipitate is formed immediately, the liquid only becoming of a fine transparent indigo blue. At length however, a black powder is deposited, leaving the liquid colourless.

Colour of gold.

This observation seems to prove, that when gold is in a state of minute division, it appears blue; and that it as-

* Two things are here taken for granted; that the precipitate is homogeneous, and that the suspended particles are precisely the same with it. From the next paragraph too it would appear, that the "slightly yellow" liquid is colourless. C.

sumes

sumes its natural colour only from the union of a certain number of its particles.

This would explain, 1st, why a very thin leaf of gold, perforated with minute holes, when held between the eye and the light appears green; because the blue colour of the most minutely divided particles mixes with the yellow of those that are less so: 2dly, why, when to a somewhat concentrated solution of gold sulphate of iron is added in sufficient quantity to reduce the whole of the gold, the liquid is of a fine green; because the yellow colour of the particles of gold united in little masses combines in some measure with the blue of those that are not yet united: and 3dly, why, in proportion as the former fall down, the liquid gradually changes to a pure blue, which it continues till the whole is precipitated. Hence it is probable, that the precipitate of Cassius does not consist wholly of metallic gold, but is rather a mixture of oxide of gold, oxide of tin, and a little metallic gold. Purple powder of Cassius.

Carbonate of potash also added to a solution of gold does not effect its precipitation, but only produces an effervescence. At the expiration of thirty hours the solution becomes turbid, without any thing separating; and it assumes a very rich red colour, in proportion as the carbonic acid it had absorbed flies off. Action of carbonate of potash on the solution.

On boiling this mixture a very thick magma is formed of the colour of pale kermes mineral; but this colour is not altered by ebullition with excess of carbonate, as is the case with caustic potash, which indicates, that the latter has some action on the precipitate. Precipitate on boiling.

When the liquid, from which the red matter was separated, appeared to have lost its colour, I filtered it, to obtain the precipitate by itself. The liquid then exhibited only a very slight tint of yellow, whence, and from its taste, which was by no means metallic but simply saline, it might have been presumed no longer to contain any gold; but this would have been a mistake. In fact a part of the liquid, into which I let fall a few drops of muriatic acid, immediately assumed a very decidedly yellow colour; and on the addition of sulphate of iron it threw down a pretty considerable quantity of metallic gold. Gold still in the liquid.

Th

The examination of this liquid I deferred, till another time, to attend to the red precipitate formed by the carbonate of potash in the solution of gold.

The precipitate examined.

I began by washing this substance with boiling water, taking care to keep each of my washings separate, that I might more easily satisfy myself when it no longer contained any thing soluble: but though I thus used a very large quantity of water in proportion to its bulk, I was never able to exhaust it; and it appeared to me, that the last washings contained nearly as much gold as the first. Hence I was led to suspect, that the precipitate was slightly soluble in water, and that, by continuing to wash it, I should perhaps cause it to disappear entirely. In consequence I ceased washing the precipitate, and dried it slowly. It greatly diminished in bulk, which proved, that it contained a large quantity of water. Its colour became a great deal deeper, and resembled that of dried blood; but when powdered it was of an orange yellow. 7·643 gram. [118 grs] of fine gold, precipipitated as mentioned above, furnished only 5·414 gr. [83.7 grs] of red matter; whence it follows, that 2·229 gr. [34·3 grs] of gold at least, or a little less than a third, remained in the mother-waters, and in the washings.

Slightly soluble in water.

Dried.

Its colour.

Only two thirds of the gold precipitated.

No excess of carbonate in it when washed.

Though I employed an excess of carbonate of potash to precipitate the solution of gold, the red matter I obtained did not contain any sensible quantity of this salt: for after it was dried, it dissolved entirely in muriatic acid without producing the least effervescence; which proves, that it had been entirely divested of carbonate by the washings, and that the precipitate it formed retained no carbonic acid.

But it retained some muriatic acid.

But it was not the same with respect to muriatic acid; for it was necessary to employ repeated portions of nitric acid, as will be seen below, to deprive the precipitate completely of the muriatic: after this the nitric solution no longer afforded a precipitate with the nitrate of silver.

Probably an oxide of gold with a little muriate.

The presence of muriatic acid in the first solutions of this matter in nitric acid led me to suspeet, that it was in the state of muriate of gold with excess of oxide; but as the latter contained no more of this acid, it appeared to me more probable, that it is simply an oxide retaining a few atoms

atoms of muriate, notwithstanding the repeated washings it had undergone.

But if potash and its carbonate precipitate in the state of oxide part of the gold dissolved in muriatic acid, why do they not precipitate the whole? and what becomes of the part left in the liquid, and in what state is it there? Why is not the precipitate homogeneous?

This we shall examine by and by: at present let us describe the properties of oxide of gold.

The oxide of gold, prepared in the manner above-mentioned, has very sensibly a styptic metallic taste, which excites the secretion of saliva copiously, and for a long time. If it be diluted with water, and blotting paper, or any other porous combustible substance, be impregnated with it, it causes them to burn with scintillation, as gunpowder would do. A decigramme [1·544 gr.] of this oxide, in a state of minute division, and shaken for some time in 60 gr. [926·7 grs] of distilled water, was not dissolved, at least entirely: the filtered liquor however, though perfectly clear and colourless, afforded with sulphate of iron a pretty copious blueish precipitate, which was metallic gold. This proves, that a solution in water had taken place: but as this solution might have arisen from some portions of salt remaining with the oxide for want of sufficient washing, I poured fresh portions of water repeatedly on the undissolved portion, and by the same means as above-mentioned found gold dissolved in them all; though it is true the proportion gradually diminished as the washings were more numerous. Though I did not dissolve the decigramme of this substance entirely, apparently because the latter portions were not sufficiently divided; I have no doubt from the little that remained, that I should at length have dissolved the whole, if I had continued my trials. Properties of the oxide of gold thus obtained.

What seems to prove it is, that the last washings, which still gave evident signs of the presence of gold, when tested with sulphate of iron, afforded no appearance of the presence of muriatic acid on adding nitrate of silver.

From these experiments we may presume, that potash, soda, and their carbonates, precipitate gold from its solution in the state of oxide; or that, at least, if any muriatic acid remain in the precipitate, it must be an infinitely small Alkalis precipitate gold as an oxide.

small quantity, when the washings have been conducted with due care.

Its medicinal qualities.

The slight solubility of this oxide, and its very easy decomposition, must render its action, as an oxigenizing substance, in the animal economy, prompt and certain.

Similar to those of the red oxide of mercury.

Oxide of silver probably analogous.

The red oxide of mercury, which has some properties in common with the oxide of gold, namely those of dissolving in water and of being easily decomposed, possesses nearly similar medicinal virtues; and from analogy we may conjecture, that oxide of silver also would have the same properties.

Action of nitric acid on the oxide.

Nitric acid does not attack dry oxide of gold, unless it be employed in large quantity, and in a concentrated state. In this it differs greatly from the muriatic acid, which dissolves it immediately. The nitric solution of gold has a brown hue; and water throws down from it a flocculent precipitate, of the same colour as that occasioned by alkalis.

The first portions of nitric acid, that have been decanted off the same oxide of gold, form a precipitate with the solution of silver, after the gold has been thrown down from them by water; but the latter portions are not precipitated, which confirms what has been said above.

The affinity of the oxide of gold for nitric acid appears very weak, for part separates in the metallic state by spontaneous evaporation. This no doubt is the reason why nitric acid alone cannot dissolve this metal.

SECT. II. *Examination of the liquor, from which gold has been precipitated by fixed alkalis.*

The liquid, from which gold had been precipitated, examined.

I have said, that this liquor has no perceptible colour, but that it resumes a pretty deep yellow, when muriatic acid is added, and that afterward an addition of sulphate of iron throws down metallic gold from it pretty copiously.

Muriate of potash first separated, then carbonate,

Having evaporated this liquor by a very gentle heat, I obtained at first crystals of muriate of potash; among which were observable some other crystals of carbonate of potash, this salt having been added in excess. The liquor being decanted from these salts, and evaporated anew with the same

same precautions, acquired a slight yellow tinge, and at length furnished a salt of the same colour, which had no regular figure. With this were mixed a few crystals of carbonate of potash perfectly colourless. The coloured salt, being well drained, produced no very decisive effervescence with muriatic acid, though the colourless crystals effervesced with it briskly; but its solution was not coloured. The coloured crystals, when redissolved in water, yielded a copious precipitate of metallic gold on the addition of sulphate of iron. The mother-water of these crystals effervesced with muriatic acid, and afterward gave a precipitate of metallic gold with sulphate of iron. and lastly a yellow salt;

These experiments seem to prove, that these crystals, as well as their mother-water, are composed of muriate of gold and muriate of potash united together in the state of a triple salt; and that the carbonate of potash is only mixed with them. being a triple muriate of gold and potash.

Hence it appears very probable, that, if a solution of gold, as nearly in the neutral state as possible, were mixed with a sufficient quantity of muriate of potash, alkalis would throw down no precipitate from this mixture. Perhaps this salt not precipitable by alkalis:

To prove this, I made the experiment above; but I obtained a precipitate with carbonate of potash: though it is true much less abundant, of a different colour, and of a different appearance, from that obtained with a solution of pure gold. Its colour was yellow, and its form granular, not flocculent like that of oxide of gold. but the conjecture not confirmed by experiment.

An examination of this precipitate informed me, that it was composed of muriate of gold, and muriate of potash rendered little soluble by the presence of alkali in the liquor, from which it had been separated. Precipitate.

One thing remarkable is, that, after having precipitated a solution of gold by means of an excess of saturated carbonate of potash, if a sufficient quantity of acid to decompose the alkaline salt be added to the filtered liquor, a few flocks of oxide of gold will be separated; and afterward, this liquor being filtered, if muriatic acid be added, it will yield a fresh precipitate by the help of boiling; but the last is a triple salt, similar to that which has just been mentioned. Oxide of gold thrown down by an acid.

I

Probably potash holds some in solution.

I think the precipitate formed by an acid in the solution of gold is to be ascribed to a small quantity of this metal held in solution by carbonate of potash. This effect takes place in a still more remarkable manner with caustic potash.

Method of obtaining the largest quantity of precipitate.

From what has been said it is evident, that, to precipitate the greatest quantity of oxide of gold possible from its muriatic solution by means of alkalis, we must manage so, that no useless acid remains in the solution; in order that less of the triple salt may be formed, on which the alkalis have no action. This is effected by evaporations to dryness very cautiously conducted.

The liquor from which gold has been precipitated should not be thrown away.

It follows too from what has been said, that the liquors, from which gold has been precipitated by alkalis, should not be thrown away, for they still contain a considerable quantity of the metal. On this occasion I may relate a curious anecdote, which shows, that many things are lost sometimes in the arts, and in manufactures, from which advantage might be derived, if we had the requisite knowledge. For many centuries jewellers had been accustomed to throw away as useless the waters, with which they cleaned their work, and thus at least two or three thousand francs were annually lost in Paris alone. But since I taught them, that these waters contained gold, and showed them the mode of getting it, they preserve them carefully.

Much thus lost by jewellers.

I am at present busy in examining the nature of the gold precipitated from its solution by metallic tin, which is also employed as a medicine; and as soon as I have finished my investigation I shall lay the result before the *Society* [of Pharmacy at Paris].

III.

Experiments on Human Bones, as a Supplement to the Paper on the Bones of the Ox: by Messrs. FOURCROY *and* VAUQUELIN*.

Magnesia supposed to exist

WHEN in the month of August, 1803, we published our first paper on the existence of magnesia in bones, we announced

* Journal de Physique, vol. LXX, p. 135.

announced, that we had not found any in human bones; and thought we might presume the cause of this difference to be the excretion of phosphate of magnesia by the urinary passages in man, while none occurs in the urine of animals. in the bones of quadrupeds only.

However, as we had made only a single experiment in search of this substance, we did not assert positively * the absence of magnesian earth in these organs.

On occasion of our last publication, in the month of September, 1808, on the presence of iron and manganese in ox-bones, we thought it necessary to resume with great care the analysis of human bones, not only with respect to magnesia, but also of the metals in question. Human bones more strictly examined,

In treating these bones in the manner we have mentioned with respect to those of the ox †, we found in them magnesia, iron, and manganese, in the same state as in the latter. yielded magnesia, iron, and manganese:

If we may be allowed to reckon on the proportions of the substances we obtained from human bones, they appeared to us to contain less magnesia, and more iron and manganese, than the bones of herbivorous quadrupeds. The small quantity of the first of these salts agrees with the continual discharge of phosphate of magnesia in the human urine. It is well known, that this is not the case with the urine of herbivorous animals: on the other hand, the iron and manganese, once entered into the course of the circulation, and deposited in the various organs of the animal economy, no longer finding an exit from the body, the quantity of these two substances apparently must increase with age, and from the known nature of food; so that the blood and bones of an old man ought to contain more iron and manganese than those of children, as well as of animals, who besides do not live so long as man. Thus the proportions with respect to quantity confirmed by our experiments are equally so by known physiological phenomena. but less of the first, and more of the other two, than those of quadrupeds.

Our last researches have shown us traces of alumine and silex likewise in human bones. The last exists in the phos- They contain also alumine and silex.

* It appears however to be asserted positively enough in the paper referred to. See Journal, vol. VIII, p. 86. C.

† See Journal, as above quoted. C.

phate

phate of ammonia resulting from the precipitation of phosphate of magnesia by volatile alkali. On evaporating to dryness, and slightly calcining the residuum, this earth is obtained of a black colour, and in a flocculent form; but by calcination at a red heat it assumes all its characteristics.

We suspected at first, that the silex and alumine might have been taken up by the phosphoric acid from the stone vessels we used: but we have since satisfied ourselves, by several decisive experiments, that they actually existed in the bones.

Though we have already given an account of the successive operations necessary for obtaining the different substances just mentioned, in the *Annales du Muséum d'Histoire naturelle* for September, 1808, we shall repeat them here, to form a complete whole, and as a guide to those who would go through the same examination.

Method of analysis.

1. Let the bones, calcined and powdered, be decomposed by an equal quantity of sulphuric acid.

2. Dilute the first mixture with twelve parts of distilled water; pour the whole on a piece of cloth, leave the sulphate of lime to drain, and wring it out strongly.

3. Filter the liquor through paper, and precipitate it by ammonia; filter it a second time, wash the precipitate, and set the liquor aside.

4. While the precipitate is still wet, treat it with sulphuric acid, taking care that the acid is a little in excess: filter afresh, wash the precipitate, and add the liquor to the former: No. 3. Repeat this operation, till the precipitate formed by the ammonia dissolves entirely in the sulphuric acid; which will show, that it no longer contains any sensible quantity of lime.

By this series of operations the whole of the lime in the bones will be converted into sulphate of lime, which, being but little soluble, will be separated from the liquor; in which will be found the phosphoric acid, with the sulphates of magnesia, iron, manganese, and alumine.

5. These substances, being separated from the sulphuric acid by ammonia, are to be treated with caustic potash, which will attract the sulphuric and phosphoric acids, evolve the ammonia, and dissolve the alumine.

6. Precipitate the alumine from the alkaline solution by means

means of muriate of ammonia, wash it, and examine by the usual means whether it be really alumine. Method of analysis.

7. Dry the magnesia, iron, and manganese, from which the phosphoric acid and alumine have been separated by the potash. Calcine them a long time in a platina crucible, and pour on them sulphuric acid diluted with water, till there is a slight excess of it.

This will dissolve the magnesia, and a portion of the iron, but not touch the manganese.

8. Evaporate the solution of magnesia containing iron, and calcine it strongly: the iron will be separated, and the magnesia, on the contrary, will remain united with the sulphuric acid. Dissolve in water, and the iron will be obtained in the state of red oxide. Precipitate the magnesia by carbonate of potash, and ascertain its purity by the usual methods.

9. Add the iron of the preceding operation to the manganese of experiment 7, and dissolve them both in an excess of muriatic acid. Dilute the solution with water, and add carbonate of potash, till a red flocculent precipitate separates, and the liquid becomes clear and colourless.

These flocks are oxide of iron. Let them be separated by filtration, and boil the liquor in a matrass. After some time, the manganese will fall down in a white powder, and when the liquor lets fall nothing more, and potash produces no effect on it, separate the manganese by filtration. Calcine it, and it will become black.

Thus the alumine, magnesia, iron, and manganese, having been separated by the means just described, nothing remains to be done but to find the silex.

10. For this purpose evaporate the liquor containing the phosphate and sulphate of ammonia of experiments 3 and 4. As it concentrates, tolerably bulky black flocks are formed, which must be separated from time to time by filtration; and when the salt is thoroughly dry, it is to be dissolved in water, and a little more of the same black matter will be obtained.

11. Wash this flocculent matter, calcine it in a platina crucible, and a white powder will be obtained, possessing all the properties of silex.

 During

During these operations the ammonia is for the most part extricated, as well as the sulphuric acid, in the state of sulphite of ammonia, and the phosphoric acid is left tolerably pure. Caustic potash however still evolves a little ammonia.

Substances found.

Thus, beside the phosphate of lime, there are in human bones, as well as in those of animals, phosphates of magnesia, iron, manganese, silex, and alumine. The last is in very small quantity; yet enough for its presence to be fully recognized and established.

These vary in their proportions.

It may be supposed, that in this method of analysis human bones will exhibit some variation in the proportions of the substances, according to the age, constitution, state of health, and general difference of the persons to whom they belonged.

The analysis very nice and difficult.

It is equally essential to observe, that, though this analysis exhibits a set of experiments simple enough in their description, it must be reckoned among the most delicate and difficult analyses, on account of the number of successive operations it includes, and the precision it requires.

IV.

Letter from Mr. BERZELIUS *to Mr.* BERTHOLLET *on the Analysis of different Salts**.

Two propositions of considerable importance to the theory of affinities.

IN studying Mr Richter's work, " On modern Subjects of Chemistry", Part I—X, 1795—1800, I found in it two propositions, which appear to me of great importance to the theory of affinities. These are: 1. That all neutral salts, which remain neutral when their solutions are mixed, are so composed, that the quantities of the different bases, that saturate one of the acids present in the mixture, follow the same proportions in saturating the other acids: 2. That a metallic neutral salt, the metal of which is precipitated by another more combustible metal, changes its metal only;

* Annales de Chim. vol. LXXVII, p. 63.

while

while the portion of oxigen, that enters into the metallic oxide, and the acid, with which it is saturated, continue the same: and that the different metallic oxides, which saturate a given portion of any acid, all contain the same quantity of oxigen.

The first of these propositions appeared to me the most important. The experiments of Mr. Richter being for the most part defective, I began by applying this principle to a great number of other analyses made by different chemists; but among these I found only six, that answered to the rule with any degree of accuracy. These were the analyses of the sulphates and muriates of barytes, potash, and soda, made by Messrs. Bucholz and Rose. The analyses of Mr. Kirwan corresponded very well with each other, but not with other analyses. The experiments I have mentioned of Messrs. Bucholz and Rose, having afforded results differing only in the thousandth parts, appeared to be the most accurate; and almost the only ones, that were sufficiently precise for inquiries of this kind. To determine this point, and in order to verify the opinion of Mr. Richter in a more decisive manner, I proposed to myself to execute a series of analyses with the most scrupulous exactitude; and for this purpose to analyse all the sulphates, and all the salts with base of barytes. From these two sets of analyses I could calculate the composition of all the other salts, and the result of this calculation remained to be confirmed by experiment. I had engaged in this pursuit in 1807, and given an account of some of the analyses in my "Elementary Treatise on Chemistry", which was published in the beginning of 1808. The truth of the principle being fully confirmed by these analyses, nothing remained, but to complete the two sets of analyses I had proposed to myself.

The first applied to various analyses.

Two sets of analyses undertaken to verify the principle.

At this juncture the discoveries of Mr. Davy on the decomposition of the fixed alkalis were published. The idea, that all salifiable bases were metallic oxides, at once struck me; and I had no doubt, that I should soon hear of Mr. Davy's having metallized also the earths and ammonia. I repeated, however, with Dr. Pontin, physician to the king, t experiments of Mr. Davy: but, as we had only a very feeble

Dr. Davy's discovery.

All salifiable bases supposed to be metallic oxides.

feeble voltaic pile, we attempted by means of a metallic conductor, fastened to the negative pole, and immersed in mercury, to collect the small portion of metallic base, that appeared to be formed. The potassium was readily deposited in it, and the little globule of mercury was reduced to a solid amalgam. We repeated the same experiment with ammonia, which was decomposed still more readily. The mercury adhering to the end of the negative conductor yielded a metallic vegetation, resembling that which is formed when a salt with base of lead is decomposed by the operation of the pile. The vegetation increased so considerably in bulk, that at length it separated from the conductor, and, floating on the liquid, was converted into ammonia with effervescence, and evolution of heat. All my endeavours to obtain this substance separate have hitherto been vain. At first I considered it as a metal composed of hidrogen and nitrogen; but the experiments of Messrs. A. Berthollet, Davy, and Henry, with which I have since become acquainted, convince me, that this opinion was unfounded. Being unable to produce this problematic substance without the assistance of mercury, I was desirous at least of ascertaining the quantity of oxigen, with which it is combined in ammonia; and perceiving the impossibility of doing it by direct experiments, I had recourse to the principle of Mr. Richter: that all bases, which saturate the same quantity of any acid, must contain the same portion of oxigen.

Attempt to form amalgam of ammonia.

Attempt to ascertain the quantity of oxigen in ammonia.

I weighed with accuracy portions of the amalgams of potassium, sodium, and calcium; I dissolved the metalloid in muriatic acid, evaporated the solution, and fused the salt in a small gold crucible. Thus I obtained results, that agreed very well with this principle. I had calculated the quantity of base in the salts from the analyses of the muriate of silver made by Messrs. Bucholz and Rose. It appeared, that 100 parts of muriatic acid saturated a quantity of potash, soda, lime, oxide of mercury, and oxide of silver, containing 42 parts of oxigen. In consequence I analysed the oxides of copper, lead, iron, and zinc; and, on combining them with muriatic acid, I believed I obtained the same results; but, after a number of tolerably accurate

Muriatic acid saturated with different oxides.

analyses, my expectation was so disappointed, that I found myself obliged to give up that principle; though, the more I reflected on it, the more probable it appeared. During my analyses of these metallic oxides I had observed another circumstance that caught my attention, namely, that the quantity of oxigen which saturated 100 parts of metal in the oxidule, was increased to half as much more, or double as much in the oxide. Thus 100 parts of lead with 7·8 of oxigen form the yellow oxidule, with 11·7 red oxide, and with 15·6 brown oxide: 100 parts of copper with 12·5 of oxigen form the red oxidule, with 25 the black oxide: &c.

Ratios of the oxigen in different oxides of the same metal

I then proposed to determine the quantity of oxigen in sulphuric and in sulphurous acid. To remove all moisture from the sulphur, I combined it with lead. I found on this occasion, that lead absorbs precisely twice as much sulphur as oxigen at its minimum of oxidation; and I soon ascertained, that it was the same with iron, copper, and tin. I am since persuaded, that the native sulphuret of iron (the *maximum*) contains for every hundred parts of iron double the quantity of sulphur that exists in the artificial (the *minimum*, magnetic iron ore). From these circumstances sulphur appears to me to follow the same laws in its combination as oxigen. It follows too, that, the composition of an oxide being known, that of the sulphuret is easily found by a simple calculation, and the contrary.

Sulphur follows the same analogy in its combination with metals.

The sulphuret of lead, oxided by nitromuriatic acid, produced a neutral salt, without either the oxide of lead or sulphuric acid predominating. 100 parts of lead combined with 15·6 of sulphur yielded precisely the same quantity of sulphate as 100 parts of lead dissolved in nitric acid, the solution being afterward mixed with sulphuric acid, evaporated to dryness, and the residuum heated redhot. From these experiments I was persuaded, that the sulphuret of lead contains precisely the quantity of sulphur necessary for the formation of the sulphuric acid required to saturate the oxide of lead yielded by the same quantity of sulphuret. Experiments on the sulphuret of iron at a *minimum*, and on the sulphate of oxidule of iron, convinced me, that the same thing took place with the sulphuret of iron.

The sulphur in a sulphuret exactly sufficient to form a sulphate.

From

General laws. From all this I deduced the following consequences; *a.* A metal combines with sulphur at a *minimum* in such a proportion, that, the sulphur being acidified, and the metal oxidulated, the result is a neutral sulphate of the oxidule; *b.* A sulphate of an oxidule contains half as much oxigen as there is sulphur in the sulphuric acid, with which it is saturated.

Composition of sulphuric acid and sulphates. From repeated experiments I have found, that sulphuric acid is composed of 40 parts sulphur, and 60 oxigen, almost precisely; and that 100 parts of sulphuric acid saturate a quantity of base containing 20 parts of oxigen. The following is an incontestible proof of the truth of this opinion, which I was on the point of giving up. On comparing the result of my experiments with that of the experiments of Mr. Bucholz, who had found 42 parts of sulphur and 58 of oxigen in sulphuric acid, I discovered, that his analysis of

Sulphate of barytes. sulphate of barytes was inaccurate. According to him this salt is composed of 32·5 acid and 67·5 base: I find it to consist of 34 acid and 66 base*. The inaccuracy of the analysis of the sulphate occasioned an inaccuracy in the analysis of the muriate of barytes, and in that of the muriate of silver. I endeavoured to correct these defects by experiments as accurate as possible, and found the muriate of

Muriate of silver. silver to be composed of 18·7 muriatic acid, and 81·3 oxide of silver. On applying these corrections to my former analyses I perceived the harmony, that I had hitherto missed. Every thing then confirmed me in the opinion, that the different bases, which saturate the same quantity of any acid, contain the same quantity of oxigen.

Sulphureus acid. On oxidating sulphite of barytes by means of nitric acid I obtained neutral sulphate of barytes, without any superfluous sulphuric acid, or nitrate of barytes, being formed. The increase of weight of the sulphite taught me, that sulphurous acid consists of almost exactly equal parts of sulphur and oxigen; or, that 100 parts of sulphur combine with near 100 parts of oxigen to form sulphurous acid, and with about 150 to form sulphuric acid. From these expe-

* For analyses of the sulphate of barytes by Mr. James Thomson and Mr Berthier, see Journal, vol. XXIII, p. 174, and 280.

riments

riments I conclude, that sulphurous acid presupposes in the bases with which it is saturated the same quantity of oxigen as sulphuric acid. It appears to me probable also, that the metal and sulphur always remain in the same proportion to each other in the sulphuret, sulphuretted oxide, sulphite of the oxidule, sulphate of the oxidule, and combination with sulphuretted hidrogen. But I have proved, that the proportion between the metal and sulphur is altered in the sulphates of the oxides, when the oxigen in the oxide is equal to that of the oxidule multiplied by 1·5.

By the analysis of the muriate of lead I found, that the base, which saturates 100 parts of muriatic acid, contains 30·49 parts of oxigen: and on calculating from this result the composition of the oxidule and oxide of copper, of the oxides of silver and lead, and of potash, soda, and lime, I always obtained results agreeing sufficiently with those of the direct experiments. The sulphates of iron, copper, lead, lime, potash, and soda, giving also, both by calculation and experiment, results corresponding with each other and with those of the muriates, I have imagined, that this point may be considered as completely settled. It is to be understood, that all these different analyses could not be carried to such perfection, as to give results not varying in the thousandths, and sometimes even in the hundredth parts; but these circumstances are to be ascribed rather to the difficulty of executing analyses with perfect accuracy, than to an erroneous principle.

Examination of muriates, and of sulphates, confirms the principle.

The oximuriatic acid combines with metals, and forms neutral salts, in which neither the acid nor oxide predominates. Hence 100 parts of muriatic acid are combined with the same quantity of oxigen in the oximuriatic acid as in the muriatic salts, that is to say, with 30·49 parts. In the experiments of Mr. Davy, potassium exposed to common muriatic acid gas was condensed, forming a neutral salt, and evolving hidrogen gas. It is evident therefore, that 100 parts of muriatic acid are combined with a quantity of water, that contains 30.49 of oxigen; that is, with 34·5 of water. Concentrated sulphuric acid contains, according to accurate experiments, almost a fifth part of water: that is to say, 100 parts of this acid are combined with 22·6 of

Oximuriatic acid.

Muriatic acid gas combined with water.

Sulphuric acid

water,

All substances, that combine with a given acid, contain a given quantity of oxigen.

water, which contain 20 parts of oxigen. It appears then, that this rule may be applied to every other substance, mineral, vegetable, or animal, which forms with acids a marked or neutral compound; for instance, the matter of the bile, albumen, and several colouring matters. In short, this law may be extended to all the acids, and every substance in any way capable of saturating them.

Composition of water.

To ascertain the composition of water I employed distilled zinc and sulphuric acid. The decomposition was performed in an apparatus accurately weighed; and the hidrogen gas was transmitted through a tube filled with muriate of lime. 200 parts of zinc yielded 248·8 of oxide, and evolved 6·5 of hidrogen gas. According to this experiment water is composed of 11·75 hidrogen and 88·25 oxigen; which agrees exactly with the experiments of Messrs. Biot and Arago. On dissolving a quantity of sulphuret of iron at a minimum in muriatic acid, I received the sulphuretted hidrogen gas in a caustic lixivium, by which it was entirely absorbed. Hence it follows, that the sulphur, which saturates 100 parts of hidrogen, must be to the oxigen, that saturates the same portion, in the same ratio as the sulphur is to the oxigen with which 100 parts of iron are saturated. The quantity of oxigen that saturates 100 parts of hidrogen being 750·77, the quantity of sulphur must be 1501·54, and sulphuretted hidrogen gas is composed of 6·243 hidrogen and 93·756 sulphur.

Sulphuretted hidrogen gas.

Composition of ammonia.

After all these experiments I thought, that a calculation of the composition of ammonia might afford a result, that would at least approach the truth. Accordingly I analysed the muriate of ammonia, and found it to be composed of 49·46 muriatic acid, 31·95 ammonia, and 18·59 water of crystallization. Consequently 100 parts of acid are saturated by 64·6 of ammonia. From analogy with the other alkalis this quantity must contain 30·49 of oxigen: and hence it follows, that ammonia is composed of 47·2 oxigen and 52·8 metallic base.

Quantity of oxigen in an acid may be deducted from that in the

It was to be presumed, that a salifiable base would determine in some measure the quantity of oxigen in the acid required for its saturation; but this proportion was not so easy to find as that between the acid and oxigen in the base.

I was

I was fortunate enough however to discover it. It is as follows. base which neutralizes it.

" In a compound formed by two oxided substances, that which, in the circuit of the electrical pile, ranges itself round the positive pole (the acid, for example) contains two, three, four, five, &c. times as much oxigen as that which ranges itself round the negative pole (for example, the alkali, earth, metallic oxide)." This law, being applicable to many other combinations beside salts, will soon impart to chemical analysis an unexpected degree of perfection. Most acids contain twice as much oxigen, as the bases that saturate them, as the carbonic acid; others three times as much, as the sulphuric acid for instance; and others, as the hyperoximuriatic acid, as far as twice* as much. In all these compounds water acts an important part: sometimes we find it uniting as a base with the acids, for instance with the crystallized vegetable acids and mineral acids; and at other times taking the place of an acid, and combining with the alkalis, earths, and metallic oxides, forming what we called hydrates. Law of the proportion.

There is every appearance, that the muriatic acid contains twice as much oxigen as the bases that saturate it. In this case it is composed of 61·3 oxigen, and 38·6 base; or 100 parts of the base combine with 156 of oxigen to form common muriatic acid, with 234 to form oximuriatic, and with 624 to form the hyperoximuriatic. Compounds of the muriatic base with oxigen.

It is but very lately, that I have had an opportunity of reading the interesting work of Mr. Gay-Lussac on the bulks of the gasses that enter into combination. It is evident, that his experiments confirm a part of the ideas, which I have had the honour to communicate to you. They contain facts, of which I have availed myelf, to acquire information on a subject, the knowledge of which it was highly important to me to obtain. According to Mr. Gay-Lussac, 100 cubic inches of carbonic oxide gas mixed with 50 cubic inches of oxigen gas produce 100 cubic inches of carbonic acid Bulks of gasses that enter into combination. Compounds of carbon and oxigen.

* In the original 2 *fois*, but the figure is palpably erroneous from the context. From the succeeding paragraph it should probably be 8 *fois*; eight times instead of twice. C.

acid gas. Consequently carbon combines with oxigen in two proportions, one of which is double the other: and as 100 parts of carbon are combined with 251·637 of oxigen in carbonic acid, they absorb 125·818 to form carbonic oxide gas. Dr. Thomson in his analysis of inflammable gasses has given the following particulars respecting carburetted hidrogen gas. 100 cubic inches of carburetted hidrogen gas consume 200 c. i. of oxigen gas, and form 100 c. i. of carbonic acid gas: 100 c. i. of olefiant gas consume 300 c. i. of oxigen gas, and form 200 c. i. of carbonic acid gas. By a very simple calculation we find, that 100 parts of carbon combine with 16·7597 of hidrogen at a *minimum*, and precisely twice as much at a *maximum*. We see by the analysis of sulphuretted hidrogen gas, already mentioned, that 100 parts of sulphur combine with 6·66 of hidrogen. If from these data we endeavour to calculate the degree of oxidation of sulphur that answers to the gaseous oxide of carbon in the following manner, 16·7597 : 125·818 : : 6·66 : 49·997, we perceive, that there is a point of oxidation of sulphur, in which 100 parts of sulphur are combined with 50 of oxigen very nearly.

Composition of inflammable gasses.

Sulphuretted muriatic acid.

On examining Mr. A. Berthollet's experiments on sulphuretted muriatic acid, if I may be allowed the term*, we see, that 100 parts of sulphur had condensed 204 of oximuriatic acid, containing 47·67 of oxigen. In the experiments of Messrs. Bucholz and Gehlen care was taken to combine with the acid the greateſt quantity of sulphur possible; and 100 parts of sulphur yielded 211 of the mixture: so that 100 parts of sulphur were combined with 25·19 of oxigen and 85·91 of muriatic acid. Admitting, that Mr. Berthollet muſt have had 214 parts of oximuriatic acid combined with 100 of sulphur, and Messrs. Bucholz and Gehlen 107 parts combined with the said quantity, we have two oxides of sulphur, one of which is composed of 100 sulphur and 25 oxigen, the other of 100 sulphur and 50 oxigen. Thus the compounds of sulphur with muriatic acid form a muriate of the oxidule of sulphur, and a muriate of the oxide. From

Two oxides of sulphur.

* Certainly Dr. Berzelius need not scruple to use the name given to this compound by its discoverer, Dr. Thomson, for whose account of it see Journal, vol. VI, p. 104. C.

this

this view of things I have concluded, that the degrees of oxidation, which appear to be multiplications by 1½, are in fact only multiplications by 6 or 12 of a degree of oxidation at a *minimum*, which is not known, because it cannot exist in a separate ſtate.

I have lately read in the Philosophical Annals of Messrs. Gilbert a paper by Messrs. Thenard and Gay-Lussac, which appears to prove, that the amalgam of ammonia is a compound of mercury, alkali, and hidrogen. I cannot however be of their opinion: for, having demonstrated by incontestible experiments the oxidation of the metalloids of potash and soda, it would be highly inconsistent to suppose, that ammonia alone ſhould exhibit phenomena so similar in outward appearance to those of the fixed alkalis, earths, and metallic oxides, while intrinsically they were of a totally different nature. I am convinced, therefore, that the substance in ammonia, which forms an amalgam with mercury in the circuit of the pile, is a metal as indecomposable as the others. But, supposing this, it naturally follows, that hidrogen and nitrogen muſt be its oxides, as Mr. Davy had already supposed*. From the laws that I have endeavoured to establish it would be easy to determine the quantity of oxigen, that enters into each. If, as I have endeavoured to prove, ammonia is composed of 100 base to 89·4 of oxigen, we shall find the quantity of oxigen, which with 100 parts of the base forms hidrogen, by dividing 89·4 by 2, 4, or 8. The quantity of oxigen necessary to convert these 100 parts of the base into nitrogen will be 89·4 multiplied by 1·5, 2, 4, &c.

Supposition, that ammonia contains no oxigen.

Hidrogen and nitrogen considered as two different oxides of the metallic base of ammonia.

We shall have found the true proportions, when the quantity of hidrogen and nitrogen gasses produced from ammonia by means of electrical discharges contain, according to these calculations, the same quantity of oxigen as ammonia. On dividing 89·4 by 8 we shall have the oxigen necessary to form hidrogen with 100 parts of the base, and on multiplying 89·4 by 1·5 we shall have the quantity required for the formation of nitrogen. On reducing the measures of gas to weights, we shall find, that 18·66 grs. of

Compounds of ammonium and oxigen.

* Dr. Davy has since been inclined to relinquish this supposition. C.

ammonia

ammonia yield 14·85 of nitrogen, and 3·81 of hidrogen, in which we shall find, according to the calculation above mentioned, 8·8 of oxigen. It is very natural, that I should hitherto have been able to obtain only tolerably near approximations to the truth, and thus trifling differences may exist, without the principle, on which I have founded this calculation, being erroneous. According to this 100 parts of the base of ammonia, which I shall call ammonium, combine with 11·175 of oxigen, to form hidrogen. This quantity I shall express by 1 *ox*. The combination of 44·7 = 4 *ox*. (oxidule of ammonium) exists in all probability in the olive-coloured substance formed by the contact of potassium with ammoniacal gas. That of 89·4 = 8 *ox*. forms ammonia: and 134·1 = 12 *ox*. forms nitrogen, which ought therefore to be composed of 57·28 oxigen and 42·72 ammonium. From the corresponding analysis of Messrs. Davy and Gay-Lussac, 100 parts of nitrogen combine with 57·3 of oxigen to form the oxidule of nitrogen [nitrous oxide]: but these hundred parts of nitrogen contain 57·3 of oxigen, so that in the oxidule of nitrogen ammonium is combined with twice as much oxigen as in nitrogen; that is, 100 parts of ammonium are combined with 268·2 = 24 *ox*. According to the analyses already quoted 36 *ox*. produce nitrous gas, and 60 *ox*. nitric acid. Between 36 and 60 the proportion of 48 is wanting, which, according to all appearance, belongs to nitrous acid. 76 *ox*. produce water, but this number stands too much alone to be verified by any calculation*.

Hidrogen evolved from

Another circumstance, that has appeared difficult to explain, is, that potassium evolves nearly the same quantity of hidrogen in ammonia as in water. If the experiments of Mr. Davy be as accurate as they appear, 100 parts of potassium

Proportion of oxigen to hidrogen in water.

* The proportion of oxigen to hidrogen in water, on which Mr. Berzelius appears to have formed his calculation, is 88·25 to 11·75: but, if we take the oxigen in water = 72 *ox*., to use his expression, which is just double 36 *ox*., the proportion of oxigen to hidrogen in 100 parts of water will be 87·7 to 12·3 very nearly. This differs but a trifle from the conclusions of von Humboldt and Gay-Lussac, adopted by Mr. Dalton in his Chemical Philosophy, Part II, p. 274, 5: and the mean between the two will be a near approximation to 87·5 and 12·5, which are precisely in the ratio of 7 to 1. C.

tassium decompose 37·8 of ammonia. The potassium then forms an oxidule, combining with 10·5 of oxigen, and it also reduces the ammonia to the state of oxidule. But these two oxidules must be combined in such proportion, that one contains twice or thrice as much oxigen as the other. If we admit, that the oxidule of potash contains thrice as much oxigen as that of ammonia, it follows from this calculation, which cannot be perfectly accurate, that the potassium must produce a quantity of hidrogen exceeding in a very trifling degree what it evolves from water. ammonia by potassium.

Table of the Analyses in the little Treatise I have the Honour to transmit to you.

Analysis of various salts.

Oxides of lead.	
—— Yellow	Lead, 100 parts: oxigen, 7·7
—— Red	—————— 11·1
—— Brown	—————— 15·4
Sulphuret of lead	———— Sulphur, 15·445
Sulphate of lead	Sulphuric acid, 100: oxide of lead, 280
Muriate of lead	Acid, 100; oxide of lead, 421·4
Carbonate of lead	Acid and water, 16·5: oxide of lead, 83·5
Sulphurous acid......	Sulphur, 100: oxigen, 99·8
Sulphuric acid	———— ——149·6
Sulphate of barytes	Acid, 100: base, 194
Sulphite of barytes	—— 86·53: water 4·25: base, 209·22
Carbonate of barytes ..	—— 21·6: base, 78·4.
Sulphuret of copper..	Copper, 100: sulphur, 25·6
Oxidule of copper	Copper, 100: oxigen, 12·5
Oxide of copper......	———— ——— 25
Sulphate of copper	Acid, 49·1: oxide, 50·9
Sulphate of oxidule of copper............	—— 100: oxidule, 183
Muriate of oxidule of copper............	—— 100: ——— 278·4
Neutral muriate of oxide of copper	—— 100: oxide, 148·7

Submuriate

Analysis of various salts.

Submuriate of oxide of copper …………	acid 100: oxide 396
Muriate of barytes ……	—— 100: barytes, 288·2
———— silver ……	{ — 18·7: oxide, 81·3 — 100: —— 434·8 }
Oxide of silver ……	Silver, 100: oxigen 7·9
Sulphuret of iron at a minimum ………	Iron, 100: sulphur 58·75
———— at a maximum ………	—— 100: —— 117
Sulphate of oxidule of iron ……………	Acid, 100: oxidule, 88
Neutral sulphate of oxide of iron ………	—— 100: oxide, 65·5
Subsulphate of oxide of iron ……………	—— 100: —— 266
Oxide of iron ………	Iron, 100: oxigen, 44·25
Oxidule of iron ……	—— 100: —— 29·5
Potash ……………	Potassium, 100: oxigen 20
Sulphate of potash ……	Acid, 100: potash, 112·35
Muriate of potash ……	—— 100: —— 179
Soda ……………	Sodium, 100: oxigen, 34·6
Sulphate of soda ……	Acid, 100: soda, 79·34
Muriate of soda ……	—— 100: ——118·627
Ammonia ………	Ammonium, 100: oxigen, 1
Muriate of ammonia ……	Acid, 49·46: water, 18·59: 31·95: or, acid, 100: base, 64·6
Lime ……………	Calcium, 100: oxigen, 39·(
Sulphate of lime ……	Acid, 100: lime, 72·41
Muriate of lime ……	—— 100: ——107·9
Barytes …………	Barium, 89·5: oxigen, 10·5
Oximuriatic acid ……	Acid, 100: oxigen, 30·49
Common muriatic acid gas ……………	—— 100: water, 34·5
Oxide of zinc ………	Zinc, 100: oxigen, 24·4
Water ……………	{ Hidrogen, 100: oxigen, 750 ———— 11·754: —— 88 }
Sulphuretted hidrogen gas ……………	———— 100: sulphur, 15 or ———— 6·247: ————

V.

Account of a Substitute for Leghorn Plait, for Hats, &c. By Mr. William Corston, *of Ludgate Hill**.

DEAR SIR,

HAVING been honoured, in May 1805, with the gold medal of the Society, for a substitute of Leghorn plait for hats, it is with great satisfaction that I am able to inform you, that this country is now beginning to reap those advantages, which I foretold to the Society six years ago, and that many hundreds of women and children are at present employed in the various parts of this kingdom, in the manufacture of this article. Manufacture of Leghorn plait flourishing.

I sold to two persons, in less than two months, upwards of 5000 scores, and had an order from a third for 2000. But this bears but a small proportion to the demand, and evinces the truth of the statement I made of the great advantages likely to result from the introdnction of this new branch of manufacture into this country.

In Joseph Lancaster's Book on Education, I have pointed out further advantages, which may be derived by the country at large, from the cultivation of waste and barren lands for the production of the material of which the British leghorn is made. This has been proved by experiments, which I have made on Bagshot Heath, by favour of the Earl and Countess of Harcourt; and in Bedfordshire, by the benevolence and public spirit of the Duke of Bedford; and on barren land in Norfolk, near my native place. Indeed no soil can be too barren for this purpose, provided the seed will lie. I have shown, that 2000 acres might be annually cultivated in the growth of this article, and that a quantity of such land might in succeeding years be brought into more productive cultivation: but I am afraid, that this plan is Application of waste land,

* Trans. of the Soc. of Arts, &c. vol. XXVIII, p. 120.

too simple to be adopted; although I cannot but yet hope, that the agricultural societies of England will turn their attention to a plan, which will bring waste lands into cultivation, and also provide employment for thousands of poor children. If government would grant 3000 acres of the land, which lies waste on Bagshot Heath, for a few years, without any fine, and afterward on an increasing rent, according to the improvements of the soil, I would raise, in *straw alone*, what should produce an article for industry, for which upwards of £20000 would be paid annually for the employment of poor children. It is a pleasing sight for Englishmen to behold the superb buildings which are appropriated as asylums for the children of our soldiers and sailors; but in times like these, how desirable is it, that buildings of only one story high ſhould be erected in populous parishes, which might answer the double purpose of schools of industry and instruction, and thereby relieve parishes from the burden of the maintenance of poor children, and also bring them up in habits of industry and sobriety! In this way thousands of children may be employed from seven years of age, until they arrive at an age sufficiently advanced to go out as servants.

and employment for poor children.

Straw manufacture.

As by the mere invention of the *splitting of a straw*, a source of employment has been discovered, which has increased the returns in that branch not less than 3 or £400000 annually, I feel myself urged to call the attention of the discerning part of the public to a new branch of industry, which I make no doubt will, in a very few years, add nearly an equal sum to the national industry, and also be a great means of bringing into cultivation thousands of acres of land now lying waste. Since the introduction of spinning by hand, no source of employment has been discovered, which promises to afford occupation to so many thousands; spinning by hand has been superseded by the inventions of machinery, but I believe it to be impossible for machinery to absorb this branch of manual industry; the only spindles, wheels, or bobbins, engaged in this work, will be, I trust, the fingers of little children.

Straw hats will always be desirable.

Some persons may endeavour to cast a shade over these expectations, by considering the prevalent attachment to the

the wear of straw hats as the whim of the day; but I believe, that the superior comfort, in summer weather, arising from the wear of a light hat in preference to a heavy one, will induce gentlemen more and more to make use of the British leghorn; and as to the predilection of ladies for hats manufactured of split straw, I think I hazard very little in considering that as established; and when to our home consumption is added a consideration of the demand for the East and West Indies, the coast of the Mediterranean and South America, I think myself very safe in asserting, that these manufactures will employ not less than 60000 children.

Our poor's rates amount to more than five millions per annum; and there can be no remedy for so great a burden equal to the setting the children of the poor to work, so that they shall earn their own bread, instead of being chargeable to the parish. It is true, that the demand for straw-plait has caused an increased quantity to be made; yet the demand is still superior to the quantity; and in the spring, the price often advances from 30 to 50 per cent beyond its fair value, even allowing sufficient profit to the poor employed, and the dealer in the article. I believe, therefore, that this branch of manufacture is still in its infancy, and that it is likely to have great permanency; and although it may, by some, be considered as an insignificant source of revenue, yet when it is considered, that Providence has given us the means of improving the agricultural state of the kingdom, in raising the raw materials, and that so many thousands of our poor may be employed in its manufacture, I trust that every assistance will be afforded to so extraordinary a source of national wealth.

Poor rates.

The manufacture of straw still in its infancy.

If any person should doubt my arguments, I will beg leave to state a fact in confirmation of my positions. I once had the curiosity to put into the scale some straw I was about to sell, and I found that it netted upwards of twenty three pounds sterling per lb. weight. If therefore, an article, which in its unmanufactured state is considered as of little worth, can, merely by the industry of children, be rendered so valuable, I think I risk very little in affirming, that by the encouragement of the *British Leghorn*, together with

The value almost wholly from the labour.

that

that of *split straw*, we gain a sure means of bringing our waste and barren lands into cultivation; and, by the employment of our poor children, we acquire an infallible means of greatly diminishing our poor's rates.

The rye should be sown on the most barren land.

In order that the British plait may equal the Italian in fineness, I particularly recommend, that the rye should be sown on the most waste and barren land, without any reference to its produce but merely of the straw, the sale of which would afford ample remuneration; and I should be happy to take the produce of from 50 to 100 acres of such land, provided it lay convenient to the place of my manufactory. By such means, the most unproductive wastes will become valuable, and a great source of advantage opened for the employment of young children, and persons incapable of hard work.

An opportunity is thus offered for benevolent persons to build cheap schools in villages, and assemble the children of the poor together, to whom literary instruction might be given, and the children enabled to earn their own bread; and the whole effected at a trifling expense.

I flatter myself, that it will give pleasure to the Society to find, that I have not neglected an object, which has merited their attention; and which will be the means of saving immense sums to this country, which have heretofore been sent abroad for the purchase of an article, which our poorest lands and feeblest people can furnish. I remain, dear Sir,

Your obliged and obedient Servant,

WILLIAM CORSTON.

Ludgate Street, *May* 10, 1810.

VI.

Correspondence of Dr. WILLIAM ROXBURGH, *of Calcutta, with Dr.* C. TAYLOR, *Secretary to the Society of Arts, &c. on various Drugs.**

SIR,

East India fever bark, swietenia febrifuga.

I HAVE the pleasure to send you a quantity of my East India fever bark, discovered by me about fifteen years ago; since which period it has had numerous fair trials in many

* Trans. of the Soc of Arts, vol. XXVIII, p. 308.

parts,

parts, which have been attended with every success that could be wished as a substitute for Peruvian bark, for which I first ventured to propose it.

A figure and description of the tree, which furnishes this bark, have been published under the name of *swietenia febrifuga*, in my account of Coromandel plants, vol. 1, page 18, table 17. It is a large timber tree, a native of the various mountainous parts of India. You will observe, that this bark possesses an agreeable odour, and from numerous experiments, which I have made with fresh bark, I have drawn the following conclusions:— Its properties.

1. That the active parts of the bark of swietenia febrifuga are much more soluble than those of Peruvian bark, particularly in watery menstruums.

2. That it contains a much larger proportion of active, bitter, and astringent powers, than Peruvian bark.

3. That the watery preparations of this bark remain good much longer than similar preparations of Peruvian bark.

4. That the spirituous and watery preparations bear to be mixed in any proportion, without decomposition.

5. That this bark, in powder, and its preparations, are more antiseptic than Peruvian bark, or similar preparations thereof.

In my practice I generally gave from twenty to sixty grains of the fine powder in substance, either in wine or water, as circumstances required, and commonly as often as Peruvian bark is usually prescribed. Method of administering it.

I recommend, that some of this bark may be sent to the fenny countries, where intermitting fevers prevail; and if it is found to answer, which I have no doubt of, it may be imported from the East Indies at so low a rate, as to render its use very general, on account of the high price of Peruvian bark. It may be imported at a low rate.

I am, Dear Sir,

Your most obedient Servant,

March 28, 1806. W. ROXBURGH.

⁂ From experiments since made in England, the swietenia bark has been found a valuable medicine in intermittent fevers, scrofula, and in disorders usually termed nervous. Experiments in England.

DEAR

DEAR SIR,

I wrote to you lately, along with my papers on the manufacture of indigo, and of some newly discovered plants which yield that drug.

A cheap resin from the saul, or shorea robusta.

It appears to me now, that it will tend to a useful purpose to put the Society in possession of samples of a very cheap resin, the produce of one of our largest and best timber trees, called by the natives of Bengal, *saul*, and by me, *shorea robusta*. It is one of the substances used in our Indian naval yards under the general name *dammer*, and is a substitute for pitch and tar.

Its uses.

To bring it to a proper consistence for such use, it is boiled up with some cheap vegetable oil, (the Hindoos being forbidden by their religion to use any animal oil), and more or less of the vegetable oil is added, according to the purpose for which it is wanted. The Society will probably find it also applicable to other uses, as it is a pure resin, cheap and plentiful: the price of it here is from three halfpence to two pence per pound. I wish to know, whether it has been yet known in England, and whether it is likely to be in demand. It will probably be useful for making sealing-wax, and for varnish.

I am, my dear Sir,

Yours very obediently,

Calcutta, Jan. 18, 1809. W. ROXBURGH.

MY DEAR SIR,

Black myrobalans,

I have now sent to you farther samples of the resin of my shorea robusta; and I have also sent a parcel of the black myrobalans, *(myrobalanus Indica)*, the origin of which has hitherto been unknown. I believe, that they are the unripe fruit of the same tree, which produces the *chebulic myrobalans*; and you will trace the cause of my now having discovered the tree which produces them in part 3 of the eleventh volume of the Asiatic Researches, containing a catalogue of Indian medicinal plants and drugs, with their names in the Hindustani and Sanscrit languages, by John Fleming, M.D., pages 29, 30, and 31, which the author sends to you for the Society. But though their medicinal virtues are in high repute over Asia, I do not send them to you with that view alone,

alone, but rather because I think they contain much *tannin* in little bulk, and may therefore be useful, and save the British oak plantations. I fear the gaub extract, from the fruit of *embryopteris glutinifera*, which I sent you some time since for the trial of the tanners, may not have answered so well as I expected, otherwise that you would have applied for more of it. They contain much tannin.

I take the present opportunity to request you will correct a mistake in my letter of June 18, 1804, published in the 23d volume of the Society's Transactions, page 408, where I said *hurra* was the fruit of *terminalia citrina*; I now find it is the fruit of *terminalia chebula*.—See Coromandel plants, 2, No. 197, and of Wildenouw's edition of the Species Plantarum, 4, 969. I now send to you a drawing and description of the tree, and of the myrobalans in their various stages, both fresh from the tree and dried as mentioned by Dr. Fleming. The small parcel within the other contains some of the drug purchased in the bazar, viz. four pounds weight, for which I paid one shilling. The remainder are fresh gathered from two trees in this garden, and hastily dried in the sun; they are rather advanced, and may answer to the fourth, fifth, and sixth sorts of the drawing; and among those of the bazar will be found the three first. I have also sent you some more fever bark, part of the produce of a young tree which grew in this garden. It is difficult to judge how long we may be conveniently supplied with Peruvian bark; and it is therefore very proper, that this valuable substitute should be brought into general use as soon as possible, and if it is likely to meet with extensive demand, I will contrive that some of it be sent home for sale. Hurra, the fruit of terminalia chebula.

In the same package is enclosed some bark of a new species of *brucea*, which is said to be a most powerful medicine; it is the *lussa radga* of Rumphius's Herbarium Amboinensis, 7, p. 27, t. 15: it is a thin bark, and may probably be as good or better than *simarouba*. In the same bundle is another parcel, which is the *conessi bark* of our *Materia Medica*; it has an austere bitter taste, and is recommended in dysenteries, diarrhœas, &c., as an astringent. New species of brucea. Conessi bark.

gent*. I wish to receive the opinion of the Society on these and other articles, which I have sent.

I am, dear Sir,

Yours very obediently,

Calcutta, Oct. 3, 1809. W. ROXBURGH.

MY DEAR SIR,

Gaub extract.

Captain Richardson having been detained thus long, and this being the season for the gaub fruit, I have made a few pounds of the extract, which is packed in the same box with the articles mentioned in my former letter. At the bottom of the box there are ten pounds made with cold water. Immediately above it is another stratum, weighing six pounds and a half, made with hot water from the refuse left after the cold water process. These two parcels, with that I sent you formerly, will certainly enable the Society to ascertain and let me know what prospect of success this extract holds out to your tanners. I request the Society will order experiments to be made therewith as early as possible, and I anxiously wait for letters from you acquainting me with the result.

I remain, dear Sir,

Yours truly,

Calcutta, Nov. 21, 1809. W. ROXBURGH.

Samples for trial.

*** Samples of the several articles above mentioned will be delivered for trial to such persons, as will engage to favour the Society with the result of their experiments thereon.

VII.

Demonstration of the Fundamental Property of the Lever, by DAVID BREWSTER, *LL. D. F. R. S. Edin.*‡

The fundamental pro-

IT is a singular fact in the history of science, that, after all the attempts of the most eminent modern mathematicians, to

* See farther particulars of this bark in Dr. Roxburgh's Treatise of Nerium Indigo, p. 254 of this volume, under the name of *Nerium Antidysentericum*.

‡ Trans. of the Roy. Soc. of Edinb. vol. VI, p. 378.

to obtain a simple and satisfactory demonstration of the fundamental property of the lever, the solution of this problem given by Archimedes should still be considered as the most legitimate and elementary. Galileo, Huygens, De la Hire, Sir Isaac Newton, Maclaurin, Landen, and Hamilton, have directed their attention to this important part of mechanics; but their demonstrations are in general either tedious or abstruse, or founded on assumptions too arbitrary to be recognised as a proper basis for mathematical reasoning. Even the demonstration given by Archimedes is not free from objections, and is applicable only to the lever considered as a physical body. Galileo, though his demonstration is superior in point of simplicity to that of Archimedes, resorts to the inelegant contrivance of suspending a solid prism from a mathematical lever, and of dividing the prism into two unequal parts, which act as the power and the weight. The demonstration given by Huygens assumes as an axiom, that a given weight, removed from the fulcrum, has a greater tendency to turn the lever round its centre of motion; and is, besides, applicable only to a commensurable proportion of the arms. The foundation of Sir Isaac Newton's demonstration is still more inadmissible. He assumes, that, if a given power act in any direction upon a lever, and if lines be drawn from the fulcrum to the line of direction, the mechanical effort of the power will be the same when it is applied to the extremity of any of these lines; but it is obvious, that this axiom is as difficult to be proved, as the property of the lever itself. Mr. De la Hire has given a demonstration which is remarkable for its want of elegance. He employs the *reductio ad absurdum*, and thus deduces the proposition from the case where the arms are commensurable. The demonstration given by Maclaurin has been highly praised; but if it does not involve a *petitio principii*, it has at least the radical defect of extending only to a commensurable proportion of the arms. The solutions of Landen and Hamilton are peculiarly long and complicated, and resemble more the demonstration of some of the abstrusest points of mechanics, than of one of its simplest and most elementary truths.

property of the lever never satisfactorily demonstrated.

Attempted by

Archimedes,

Galileo,

Huygens,

Sir I. Newton,

De la Hire,

Maclaurin,

Landen, and Hamilton.

In attempting to give a new demonstration of the fundamental

A new demon-

stration attempted. mental property of the lever, which shall be at the same time simple and legitimate, we shall assume only one principle, which has been universally admitted as axiomatic, namely, *that equal and opposite forces, acting at the extremities of the equal arms of a lever, and at equal angles to these arms, will be in equilibrio.* With the aid of this axiom, the fundamental property of the lever may be established by the three following propositions.

Axiom.

Propositions. In Prop. I, the property is deduced in a very simple manner, when the arms of the lever are commensurable.

In Prop. II, which is totally independent of the first, the demonstration is general, and extends to any proportion between the arms.

In Prop. III, the property is established, when the forces act in an oblique direction, and when the lever is either rectilineal, angular, or curvilineal. In the demonstrations which have generally been given of this last proposition, the oblique force has been resolved into two, one of which is directed to the fulcrum, while the other is perpendicular to that direction. It is then assumed, *that the force directed to the fulcrum has no tendency to disturb the equilibrium, even though it acts at the extremity of a bent arm*; and hence it is easy to demonstrate, that the remaining force is proportional to the perpendicular drawn from the fulcrum to the line of direction in which the original force was applied. As the principle thus assumed, however, is totally inadmissible as an intuitive truth, we have attempted to demonstrate the proposition without its assistance.

General assumption here dispensed with.

Prop. I.

Prop. I. *If one arm of a straight lever is any multiple of the other, a force acting at the extremity of the one will be in eqnilibrio with a force acting at the extremity of the other, when these forces are reciprocally proportional to the length of the arms to which they are applied.*

Demonstrated. Let AB (Plate VIII, fig. 1,) be a lever supported on the two fulcra F, f, so that Af=fF=FB. Then, if two equal weights C, D, of 1 pound each, be suspended from the extremities A, B, they will be in equilibrio, since they act at the end of equal arms Af, BF; and each of the fulcra f, F, will support an equal part of the whole weight, or 1 pound.

Let

Let the fulcrum f be now removed, and let a weight E, of 1 pound, act upwards at the point f; the equilibrium will still continue; but the weight E, of 1 pound, acting upwards at f, is equivalent to a weight G of 1 pound, acting downwards at B. Remove, therefore, the weight E, and suspend the weight G from B; then, since the equilibrium is still preserved after these two substitutions, we have a weight C, of one pound, acting at the extremity of the arm AF, in equilibrio with the weights D and G, which together make two pounds, acting at the extremity of the arm FB. But FA is to FB as 2 is to one; therefore an equilibrium takes place, when the weights are reciprocally proportional to the arms, in the particular case when the arms are as 2 to 1. By making Ff successively double, triple, &c. of FB, it may in like manner be shown, that, in these cases, the proposition holds true.

Prop. II.

Prop. II. *If two forces, acting at the extremities of the two arms of a lever, and at equal angles to the arms, are in equilibrio, they will be reciprocally proportional to the lengths of the arms to which they are applied.*

Demonstrated.

Let AB, CD (fig. 2,) be two levers in contact at AB, and forming one straight line, ABCD. Bisect AB in f, and CD in φ, and from the extremities A, B, suspend equal weights m, m, and from the extremities C, D, equal weights n, n, so that $m:n=\text{CD}:\text{AB}$. If the two levers are now supported on the fulcra f, φ, they will both be in equilibrio, and will still form one straight line, the fulcrum f being loaded with a weight$=2\,m$, and the fulcrum φ with a weight$=2\,n$. Let us now suppose the extremities B, C, of the levers to adhere, and form one inflexible line AD; and let an inverted fulcrum F be placed at the point of junction. The equilibrium of the whole will evidently continue, and the fulcra f, φ, will be loaded as before. Remove the fulcra f, φ, and substitute in their place the weights $2\,m$, $2\,n$, acting upwards, and equal to the load which they respectively support: The equilibrium will still continue. Now, instead of the force m acting downwards at B, substitute an equal and opposite force m', acting upwards at A, and instead of the force n acting downwards at C, substitute an equal and opposite

posite force n', acting upwards at D, and the equilibrium will still be preserved. But the two equal forces acting in opposite directions at the points A and D destroy each other; therefore we have a force $2m$ acting at the extremity of the arm fF, in equilibrio with a force $2n$, acting at the extremity of the arm ϕF. But since, by the hypothesis, $m:n$ as CD : AB, and since fF is one half of AB, and ϕF one half of CD, we have $2m:2n = \phi F : fF$, an analogy which expresses the fundamental property of the lever.

Lemma. LEMMA. *Two equal forces acting at the same point of the arm of the lever, and in directions which form equal angles with a perpendicular drawn through that point of the arm, will have equal tendencies to turn the lever round its centre of motion.*

Demonstrated. Let AB (fig. 3,) be a lever with equal arms AF, FB. Through the points A, B, draw AD, BE, perpendicular to AB, and AP, Ap, BW, Bw, forming equal angles with the lines AD, BE. Produce PA to M. Then, equal forces acting in the directions AP, Bw, will be in equilibrio. But a force M equal to P, and acting in the direction AM, will counteract the force P, acting in the direction AB, or will have the same tendency to turn the lever round F; and the force W, acting in the direction BW, will have the same tendency to turn the lever round F as the force M; consequently the force W will have the same tendency to turn the lever round F as the force w.

Prop. III. PROP. III. *If a force acts in different directions at the same point in the arm of a lever, its tendency to turn the lever round its centre of motion will be proportional to the perpendiculars let fall from that centre on the lines of direction in which the force is applied.*

Demonstrated. Let AB, (fig. 4,) be the lever, and let the two equal forces BM, Bm, act upon it at the point B, in the direction of the lines BM, Bm. Draw BN, Bn, respectively equal to BM, Bm, and forming the same angles with the line PBω perpendicular to AB. To BM, Bm, BN, Bn, produced, draw the perpendiculars AY, Ay, AX, Ax. Now, the side AX = AY, and Ax = Ay, on account of the equality of

of the triangles ABX, ABY; and if $M\,l$, $M\,\lambda$, be drawn perpendicular to $B\,\omega$, the triangles ABY, $BM\,l$, will be similar. and also the triangles $AB\,y$, $B\,m\,\lambda$: Hence we obtain

$$AB : AY = BM : B\,l, \text{ and}$$
$$AB : A\,y = BM : B\,\lambda$$

Therefore, *ex æquo*, $AY : A\,y = B\,l : B\,\lambda$

Complete the parallelograms $BM\,o\,N$, $B\,m\,\omega\,n$; and $B\,l$, $B\,\lambda$ will be respectively one half of the diagonals $B\,o$, $B\,\omega$.

Now let two equal forces BM, BN, act in these directions upon the lever at B, their joint force will be represented by the diagonal $B\,o$, and consequently one of the forces BM will be represented by $B\,l = \frac{1}{2}\,B\,o$. In the same manner, if the two equal forces $B\,m$, $B\,n$, act upon the lever at B, their joint force will be represented by $B\,\omega$, and one of them, $B\,m$, will be represented by $B\,\lambda = \frac{1}{2}\,B\,\omega$. Consequently the power of the two forces BM, $B\,m$, to turn the lever round its centre of motion, is represented by $B\,l$, $B\,\lambda$, respectively; that is, the force BM is to the force $B\,m$ as $B\,l$ is to $B\,\lambda$; that is, as AY is to $A\,y$, the perpendiculars let fall upon the lines of their direction.

VIII.

On the Nature of those Meteors commonly called Shooting Stars. In a Letter from JOHN FAREY, *Senior, Esq.*

To WILLIAM NICHOLSON, Esq.

SIR,

IN several Meteorological Reports of late, and particularly in a letter from Mr. Thomas Forster, at page 131 of your October number, the appearances usually denominated *shooting stars* are noticed, and treated of as being a phenomenon connected with the electric and other particular state of our atmosphere, particularly "clear *dry* weather "and easterly winds," "clear frosty winter nights," and "the clear intervals of showery weather." Now these three states

Shooting stars supposed to be connected with electrical or other states of our atmosphere.

Its clearness apparently ne-

cessary to our seeing them.

states of the air are best adapted, by its clearness, for seeing the smaller stars and planets, or any small distant object by land; and I wish particularly to call the attention of your Meteorological Correspondent to notice, whether the absence of the *twilight, moon-light*, &c. is not equally essential to seeing numbers of the small rapidly shooting-stars; as I certainly found them to be, in a series of observations continued for more than a year in 1800 and 1801, in conjunction with an able friend at 6 miles distance; and whence it seemed ascertained, that these phenomena are occasioned by an almost infinite number of *satellitulæ*, or very small moons, constantly revolving round the Earth, in all possible directions, and appearing only during the very short time that they dip into the upper part of the atmosphere each time that they are in *perigee*: and that no step seems wanting in the degree of this dip into the atmosphere, and their consequent brightness, length, and slowness of courses, &c. between the smallest instantaneous *shooting-stars*, and the largest *meteors*, (such as that of August, 1783, alluded to by your correspondent,) which throw off with explosions angular fragments of metallic and stony matters, that so frequently fall to the Earth, as *meteoric stones*. The long trains or streaks of light, often mentioned as *left* by meteors for some instants, will frequently be found mere optical deceptions, owing to the eye not following the meteor, but suffering it to cross the field of sight, where its impression is left, on known optical principles.

Probably they are satellitules, of which an infinite number move in all directions round the Earth,

from the smallest to the largest meteors.

Hoping to see this important class of phenomena more closely and extensively investigated than they hitherto have been,

I remain, Sir,

Your obedient humble Servant,

JOHN FAREY, Sen.

Upper Crown Street, Westminster,
5th Nov. 1811.

IX.

IX.

On the Causes of the Decay of the Timber in Ships, and the Means of preventing it. In a Letter from a Correspondent.

To WM. NICHOLSON, Esq.

SIR,

THE advantages that England derives from her marine, whether considered as appertaining to commerce or defence, are too well known to need any comment; whatever then will contribute either to the safety or durability of the navy becomes a matter of great public importance. Shipping of great importance to this country.

The grand cause of the decay of the timber employed in building of ships is the decomposition of its substances by putrefaction, which is occasioned by moisture. This precautions and management may retard, but not prevent; but a secondary one, the dry rot, may, I think, be both prevented and eradicated. Causes of the decay of the timber of.

The dry rot, as it is usually called, proceeds from the growth of a parasitical plant, named by botanists *boletus lachrymans*, which belongs to the class of cryptogamia. Its injurious tendency is mentioned as far back as history will carry us, and the appearance and ravages are particularly pointed out in the Bible*. The cure there directed is, to remove the materials injured; and, if this did not stop the disease, the house was razed, and the entire articles of which it was composed taken without the city. In latter times an equally effectual but more easy remedy has been applied in buildings, where this plant has taken root; that of causing a circulation of air in the parts affected; but this cannot be introduced in the fabrics of which we are now treating. The dry rot known in very ancient times. Remedies for it.

The fatal tendency of the dry rot in ships cannot be pointed out in a more forcible way, than it is in the memoirs of Pepys, who was secretary to the Admiralty during the reigns of Charles the 2nd, and James the 2nd. At that time a commission was formed to inquire into the state of the Its injuriousness to the navy formerly,

* Leviticus, Chap. 14.

the

the navy, by which it appeared, that there were thirty ships called new ships, which, as he observes, "for want of proper care and attention, had toadstools growing in their "holds as big as his fists, and were in so complete a state of "decay, that some of the planks had dropped from their "sides." From that time to the present, the evil has in some measure existed; and, although it has not since appeared in so great an extent as it then did, yet the state of

and at present.

some ships recently launched both justifies and demands all possible inquiry as to the causes of the growth of this fungus, and its prevention.

Attempts to remedy it

Several means have been tried to prevent its vegetating, many of which might have answered this purpose, had they not been found to introduce evils as great as that which they pretended to cure. Among the most prominent, was the mode practised on the timbers of

by salt

many ships, between the years 1768 and 1773, by saturating them with common salt; but this was found to cause a rapid corrosion in the iron fastenings, and the ships were (between decks) in a continual state of damp vapour. Mundic, found in the mines in Devonshire, has been lately em-

and mundic.

ployed, in fusion, to eradicate the vegetation, and prevent its future growth; but time is required to prove its efficacy.

Production of carbonic acid gas injurious.

In the common mode of constructing ships there are several causes, which promote the growth of fungi. The accumulation and consequent fermentation of materials not sufficiently seasoned, divested too of a free circulation of air, and permitting sap to remain on the edges of the frames, generate carbonic acid gas to the prejudice of the timber, and which promotes the growth of this boletus. Mr. Humboldt has found by experiments, that eight or ten hundredths of carbonic acid gas, added to the air of the atmosphere, rendered it extremely fit for vegetation; and that the air in mines, and other subterraneous passages, was found in this state, which is very favourable to the germination of all plants of the class cryptogamia. The gas found in the openings between the timbers of ships affected with the dry rot has been proved, to be precisely what Mr. Humboldt has mentioned.

Means proposed to pre-

The means, that I propose to prevent or cure this evil, are twofold: charring the whole surfaces of the timbers, and the

the inner surfaces of the planks, of which the ships are composed; and causing some slight deviations to be made in the modes practised in building them. I do not pretend to originality, when I recommend charring of timber, either to add to its durability, or prevent the growth of parasitical plants; for the experience of ages has proved the incorruptibility of charcoal, whether buried in the earth, or exposed to the action of air or water. The beams of the theatre of Herculaneum, which were reduced to this state by lava, were found, after a period of nearly eighteen centuries, to be perfect. The piles, supposed to have been driven into the earth by order of Julius Cæsar, when he forded the Thames at Cowey Stakes, near Shepperton, were charred, and, when recently taken up, found in a complete state, free from decay! Among many other instances, that may be adduced, the practice, almost universally* adopted, of burning the ends of posts to be put into the ground, to prevent premature dissolution, may be added as an additional proof of the efficacy of this recommendation; and makes us lament, that it has not been generally introduced in fabrics, where so much timber, labour, and money, have been expended; and the hopes and expectations of government or individuals frequently disappointed, by their rapid decay.

vent or cure the dry rot. Charring the surface of the timber. not an original idea.

Proofs of its efficacy in rendering timber durable.

There are several other advantages, that will be obtained by burning the surfaces of timber. Rats, which are so destructive to ships, will not touch charcoal; nor will the white ants and cockroaches, so common in the Indies, commit their depredations on substances so prepared. If farther evidence of its utility, when employed only on a small scale, be necessary, the durability of the Royal William, the flag ship at Spithead, which was built in the year 1719, and the planks *only* were burned on their inner surfaces, would be sufficient to prove its efficacy when practised on ships.

Other advantages from it.

Instances of the utility of its partial application in ships.

* I am inclined to think, that the writer is mistaken here; and that the practice is very far from being even almost *generally* adopted. I remember a year or two ago speaking of it to a carpenter, who was putting down some posts; and he observed, that it would make them last too long, an object they never had in view in parish work. He added, that they sometimes charred the ends of posts, or more frequently dipped them in tar, for a private customer, "if he particularly desired it". C.

ships. Of late years the ends of ships' beams have been charred, and the sound state in which they are now found has justified and established the practice. Indeed all substances, that have undergone the action of fire, have been proved to be unfavourable to the growth of the boletus lachrymans; for, while stone has been rapidly destroyed by it, well burnt bricks, in the same buildings, and in nearly the same situation, have been free from its attacks.

From the scarcity of oak substitutes have been employed, chiefly pine.

The scarcity of English oak, occasioned partly by the improved state of agriculture, but more by the increased numbers of our fleet, has obliged this country to have recourse to wood grown in other states. The principal that have been introduced in aid of oak are the varieties of American pine: it becomes therefore of some importance to inquire, which sort of this timber is the most durable, and which the soonest destroyed by vegetation. Pitch pine has been used by all nations in the construction of ships, and appears to be very superior to every other species for general durability; but this wood is the soonest destroyed by fungi, as these plants are nourished by the great quantity of resin contained in its numerous cells. I have lately seen some pitch pine plank of 7 inches in thickness completely decomposed; and, when cut open, the boletus was found to be vegetating in every part of it, but principally in the cells which were originally filled with resin. This proves how improper it will be to employ it as treenail fastenings, on which the strength and safety of ships so much depend. Pitch pine should not be covered with paint, as the pores of the wood are thereby stopped, and the expansion of the resin prevented, by which means the ligneous cells are broken, and decomposition takes place. The Americans pay the topsides of their ships with a mixture of oil, resin, &c., which are not unlike the substances that are contained in the wood they cover, and produce a hard varnish, impervious to water. Perhaps the preparation recommended by doctor Parry*, to prevent the dry rot, given in the Transactions of the Bath and West of England Societies, might be

Pitch pine generally durable,

but the soonest destroyed by fungi.

Very improper for treenails.

Pitch pine should not be painted.

Preferable applications.

* It is made as follows: take 12 ounces of resin, 8 of roll brimstone, 3 gallons of oil, and 4 ounces of bees wax: boil them together, and lay them on while hot. [See Journal, vol XIX, p. 337.]

be introduced also for this purpose with success. White wash or lime water to be used between decks is much to be preferred to paint, both on account of its cheapness and cleansing qualities, and also as it is detrimental to vegetation.

Whitewashing between decks recommended.

Instead of the frames of a ship being converted to their proper shape for some months before they are put up, and afterward standing on a slip a year to season, as is now the usual practice; I would recommend, that they should be converted, and remain, together with the planks, in that state (under cover where there shall be a free circulation of air) for two years, then charred, put up, and the planking immediately begun; commencing operations from within board, by which means chips and dirt will not accumulate between the timbers; care being taken, that the holes be not bored too near the seams in the outboard plank. Holes should be bored, but no treenails driven till within a short time of the ship's being launched; this will both convey air within board, and carry off the vegetable juices, if any remain in the interior of the timbers. The planks could be kept in their places by the usual butt bolts, and some copper nails, or small bolts ragged, being driven at intermediate spaces. This too would strengthen the ship, as metallic fastenings are always to be preferred, in the wales and bottom, to treenails.

Alterations proposed in the mode of building ships.

An objection may be made to the bringing round thick planks in the bow of a ship by burning, rather than the usual practice of boiling them in a kiln, on account of breaking their fibres. Although I do not see, that any difficulty can exist in the former method, as it is the usual practice of the French; yet, if any should occur on trial, and boiling them be considered absolutely necessary, vegetation may be prevented by dissolving some green vitriol in the water, and afterward fixing it in the wood by a weak alkali*. The method approved of by many judicious shipwrights, and constantly practised by the Dutch, that of

Remarks on bending the bow planks.

* A solution of alum might also be tried—[I should apprehend any saline impregnation of the planks would prove injurious to the copper sheathing and fastenings. C.]

A trial might be instituted on a small scale.

sawing the thick planks, that are to be much bent, into two parts, might also be employed for this purpose. If a doubt should exist of the efficacy of wood prepared in these several ways to prevent the dry rot, specimens might be placed in a ship almost destroyed thereby, such a one as I recollect to have seen at Woolwich about 13 years since, which was in so bad a state, that the decks sunk with a man's weight, and the orange and brown coloured fungi were hanging in the shape of inverted cones from deck to deck. A few months trial of wood put into a ship so infected would prove the efficacy of either mode of preparation.

Exsiccation of timber in an oven, as recommended by Fourcroy, is also likely to add considerably to its durability.

One farther precaution is necessary. After a ship is built, she should lie at least six months in ordinary, with her hatchways covered to prevent the admission of rain water; some planks should be removed in the ceiling, and above the waterways of the several decks; and fires constantly kept in stoves, placed in the hold, and on the decks; by which means the moisture, that the charcoal may have attracted, will be dissipated, and the durability of the fabric insured.

Having stated these general circumstances with a view to prevent evils, which yearly exist to a great extent in the navy; I trust it will be the means of calling forth the opinions and abilities of those, whose minds have been directed, or whose occupations may lead them, to a consideration of this important subject.

I am, &c. &c.

11*th of November*, 1811. NAUTICUS.

X.

On the Art of Coating Metals with Platina: by Mr. Guyton-Morveau.*

Platina may be applied on other metals in two ways.

THE application of platina on other metals less valuable, to prevent their oxidation, may be considered under two points of view, or as two different arts. The first of these

* Ann. de Chim. vol. LXXVII, p. 297.

may

may be called *platining* [*platinure*], as we say gilding, silvering: the other plating, a term appropriated by custom to a less superficial application, requiring a different process.

Platining may be executed like gilding, either by the intervention of mercury, or by means of a solution of muriate of platina in ether. That of washing.

1. I long ago made known the possibility of forming an amalgam of platina, and described the processes for obtaining it*. Mr. Proust, in a letter addressed to Mr. Vauquelin, inserted in the Ann. de Chim. pluviose, an. 12 [February, 1804], has said, that "hot mercury poured on the spongy substance remaining after the calcination of ammoniacal muriate of platina, dissolves it perfectly; and the result is a fatty amalgam, that does not grow hard by keeping, and spreads well on copper, gold, or silver; so that it might facilitate the plating of the former." By means of amalgam. Amalgam made by Proust,

From the note following this passage it appears, that Messrs. Fourcroy and Vauquelin also accomplished this amalgamation by the same process; that they even effected it without heat, and that, after having remained fluid for some time, it became very solid; an effect that might be accelerated by the application of a gentle heat. Fourcroy and Vauquelin,

Lastly, Mr. Hatchett published in Nicholson's Journal for October, 1804, a Letter, in which Count Mussin Poushkin gave him the particulars of the processes of amalgamation, by means of which he rendered platina perfectly malleable. and Count Mussin Poushkin.

At present therefore we cannot question the union of platina with mercury by means of simple processes, not expensive, and producing a suitable consistency for a solid application of the fixed metal: but it does not appear, that the processes of this new art have hitherto been published [in France] with any details; I shall therefore make known those described by Mr. Trommsdorff in the 7th vol. of his Journal, from the communication of Mr. Strauss†.

* * * * * *

* Ann. de Chim. January 1798, vol. XXV, p. 14, and fol.

† See also Nicholson's Journal, vol. IX. [As the account that follows in the text is the same with that given at p. 303 of the vol. of the Journal quoted by Mr. Guyton-Morveau, it is omitted here as unnecessary. C.]

II.

By means of ether.

II. Another kind of platining, which appears particularly adapted to similar works of polished steel or iron, to prevent their rusting, is that which results from the application of platina to their surface by means of ether.

It is well known, that, if a solution of gold in nitromuriatic acid be covered with sulphuric ether, and the two liquids shaken together, the ether will take the gold from the acid, acquire a yellow colour, and become capable of producing a true gilding, when applied to the surface of another metal.

The celebrated Lewis said, that platina would not form this union. Mr. Stodart supposes, that, if he did not effect the decomposition of muriate of platina by means of ether, it was probably because his platina was impure; and he has published in Nicholson's Journal* the process that succeeded in his hands.

* * * * *

Of plating or casing with platina.

Coating with platina.

From what has been said it appears, that the art of platining is not more difficult than gilding, and that it will have nearly the same advantage of preserving from rust those metals that are most liable to it. But at the same time it cannot be denied, that so thin a covering is far from promising the same durability, as that which is termed plating; particularly with respect to vessels and instruments continually exposed to the action of fire, or to being frequently rubbed.

Tried with success on copper 16 years ago.

I do not know, that plating with platina has yet been attempted in the large way: but there is every appearance that it would succeed as well as plating with gold or silver, and by the same well known processes. As a proof of this I have a small vessel in the shape of the bowl of a spoon, which was given me fifteen years ago by Professor Chabaneau, on his return from Spain, where he first introduced the principles of modern chemistry in his lectures†.

* Vol XI, p. 282. [What follows in the text is omitted for the same reason as in the preceding instance. C.]

† Elementos de Ciencias naturales, &c. Madrid, 1790.

This

This vessel, 75 mil. [2·951 inch.] long, by 52 [2·046 in.] broad, and 14 [0·551 in.] deep, is made of copper, plated in the inside with platina. The thickness of its edges is 0·78 mil. [0·3 of a line]; it weighs 345·05 dec. [532·95 grs.]; and its specific gravity is 11·44.

As the metals here are only in juxtaposition, which can neither increase nor diminish their density, their respective proportions may be determined with precision from their specific gravity; and if we estimate that of platina at 21, and that of copper at 8·87, we shall find by calculation, that the vessel is composed of

Copper	0·766
Platina	0.234.

Thus the plating metal is a little more than a fifth, or in the most usual proportion of silver plating, the durability of which is established by use; though the properties of this metal in resisting the actions of heat and saline substances are very inferior to those of platina.

XI.

Experiments and Observations on the different Modes in which Death is produced by certain vegetable Poisons; By B. C. Brodie, *Esq. F. R. S. Communicated by the Society for promoting the Knowledge of Animal Chemistry.**

1. THE following experiments were instituted with a view to ascertain, in what manner certain substances act on the animal system, so as to occasion death, independently of mechanical injury. I was led to the inquiry, from the subject of it appearing to be of considerable interest and importance, and from a hope, that, in the present improved state of physiological knowledge, we might be enabled to arrive at some more satisfactory conclusions, than had been deduced from any former observations. Object of the following experiments.

The substances, which act as poisons when applied to the animal body, are very numerous. In the experiments, which I have hitherto made, I have employed vegetable poisons Confined to vegetable poisons.

* Philos. Trans. for 1811, p.178.

sons only. Of these I have selected such, as are very active and certain in producing their effects, believing that, on this account, the exact nature of those effects would be more readily ascertained. The principal objects, which I have kept in view, have been to determine, on which of the vital organs the poison employed exercises its primary influence, and through what medium that organ becomes affected. I have also endeavoured to ascertain by what means the fatal consequences of some poisons may be prevented. With some of the conclusions, which I have ventured to draw, so far as I know, we were not before acquainted; and others of them, though not entirely new, had not been previously established by satisfactory experiments.

1st applied to the tongue, or taken internally.

I shall relate first those experiments, in which poisons were applied internally, that is to the mucous membranes of the tongue or alimentary canal, and afterward those, in which poisons were applied to wounded surfaces.

II. *Experiments with Poisons applied to the Tongue or alimentary Canal.*

Experiments with Alcohol.

Effects of alcohol.

When spirits are taken into the stomach, in a certain quantity, they produce that kind of delirium, which constitutes intoxication: when taken in a larger quantity, it is well known, that they destroy life altogether, and this in the course of a very short space of time. Intoxication is a derangement of the functions of the mind; and, as these are in some way connected with those of the brain, it seems probable, that it is by acting on this organ, that spirits, when taken into the stomach, occasion death. In order to ascertain how far this conclusion is just, I made the following experiments*.

* I am indebted to Dr. E. N. Bancroft for his assistance in many of the experiments, which I am about to detail. Mr W. Brande lent me his assistance in the greater part of those which were made. I have been farther assisted by Mr. Broughton, Mr. R. Rawlins, and Mr. R. Gatcombe, and by several other gentlemen.

Experiment

Experiment 1. I poured two drachms of proof spirits down the œsophagus of a cat. Instantly he struggled violently; then lay on one side, perfectly motionless and insensible; the breathing was laboured and stertorous, and the pulsations of the heart were very frequent. He continued in this state for seven or eight minutes; then began to recover; the respirations became easier, and presently he stood up, and was able to walk. Experiment 1.

Exp. 2. I injected an ounce and a half of proof spirits into the stomach of a large full-grown rabbit, by means of an elastic gum tube passed down the œsophagus. The same symptoms took place as in the last experiment; but the animal did not begin to recover from the state of insensibility, until forty minutes had elapsed from the time of the injection. Experiment 2.

Exp, 3. Seven drachms of proof spirits were injected into the stomach of a younger rabbit. Two minutes afterward, he evidently was affected by the spirits, and in three minutes more he lay on one side motionless and insensible. The pupils of the eyes were perfectly dilated; there were occasional slight convulsive motions of the extremities; the respiration was laborious, it was gradually performed at longer and longer intervals, and at the end of an hour and fifteen minutes had entirely ceased. Two minutes after the animal was apparently dead, I opened into the thorax, and found the heart acting with moderate force and frequency, circulating dark coloured blood. I introduced a tube into the trachea, and produced artificial respiration by inflating the lungs, and found that by these means the action of the heart might be kept up to the natural standard, as in an animal from whom the head is removed. Experiment 3.

Exp. 4. I injected into the stomach of a rabbit two ounces of proof spirits. The injection was scarcely completed, when the animal became perfectly insensible. Precisely the same symptoms took place as in the last experiment; and at the end of twenty-seven minutes, from the time of the injection, the rabbit was apparently dead; but on examining the thorax the heart was found still acting, as in the last experiment. Experiment 4.

It

The brain not directly necessary to the action of the heart.

It has been shown by Mr. Bichat, and the observation has been confirmed by some experiments, which I have lately had the honour of communicating to this learned Society*, that the brain is not directly necessary to the action of the heart; and that, when the functions of the brain are destroyed, the heart continues to contract for some time afterward, and then ceases only in consequence of the suspension of respiration, which is under the influence of the brain.

Spirits act on the brain,

It would appear from the experiments, which I have just detailed, that the symptoms, produced by a large quantity of spirits taken into the stomach, arise entirely from disturbance of the functions of the brain. The complete insensibility to external impressions, the dilatation of the pupils of the eyes, and the loss of motion, indicate, that the functions of this organ are suspended; respiration, which is under its influence, is ill performed, and at last altogether ceases; while the heart, to the action of which the brain is not directly necessary, continues to contract, circulating dark coloured blood for some time afterward.

and affect it in the same way as external injuries.

There is a striking analogy between the symptoms arising from spirits taken internally, and those produced by injuries of the brain.

Concussion of the brain, which may be considered as the slightest degree of injury, occasions a state of mind resembling intoxication; and the resemblance in some instances is so complete, that the most accurate observer cannot form a diagnosis, except from the history of the case. Pressure on the brain, which is a more severe injury than concussion, produces loss of motion, insensibility, dilatation of the pupils; respiration becomes laboured and stertorous, is performed at long intervals, and at last altogether ceases, and the patient dies.

Do they act by absorption, or sympathy?

It forms an interesting matter of inquiry, whether spirits, when taken into the stomach, produce their effects on the brain by being absorbed into the circulation, or in consequence of the sympathy, that exists between these organs by means of the nerves. The following circumstances lead me to conclude, that they act in the last of these two ways.

* See Journal, vol. XXIX, p. 339.

1. In

1. In experiments where animals have been killed by the injection of spirits into the stomach, I have found this organ to bear the marks of great inflammation, but never found any preternatural appearances whatever in the brain. 2. The effects of spirits taken into the stomach in the last experiment were so instantaneous, that it appears impossible that absorption should have taken place before they were produced. 3. A person who is intoxicated, frequently becomes suddenly sober after vomiting. 4. In the experiments, which I have just related, I mixed tincture of rhubarb with the spirits, knowing from the experiments of Mr. Home and Mr. William Brande, that this, when absorbed into the circulation, was readily separated from the blood by the kidneys, and that very small quantities might be detected in the urine by the addition of potash; but, though I never failed to find urine in the bladder, I never detected rhubarb in it. Most probably the latter.

The including the termination of the thoracic duct in a ligature does not prevent spirits, when taken into the stomach, from producing their usual effects on the nervous system; but subsequent observations, which Mr. Home has already communicated to this Society*, have shown, that no conclusion can be drawn from this experiment.

That a poison may affect a distant organ, through the medium of the nerves, without entering the circulation, is proved by the well-known circumstance of solution of the extract of *belladonna*, when applied to the tunica conjunctiva of the eye, occasioning dilatation of the pupil of the same eye, though no other part of the system is affected. A poison may act without entering the circulation.

It has been formerly supposed by Dr. Mead and other physiologists, that a poison may produce death by acting on the extremities of the nerves of the stomach and intestines, without being absorbed into the circulation. That it should by these means be capable of affecting the brain is not to be wondered at, when we consider the numerous and various sympathies between this organ and the alimentary canal, evidently independent of any other communication than the nerves. Supposed capable of producing death this way.

* See p. 178, of our present vol.

Experiments

*Experiments with the Essential Oil of Bitter Almonds**.

Effects of oil of bitter almonds. Experiment 5.

Experiment 5. One drop of the essential oil of bitter almonds was applied to the tongue of a young cat. She was instantly seized with violent convulsions; then lay on one side motionless, insensible, breathing in a hurried manner; the respirations became laboured, took place at longer and longer intervals, and at the end of five minutes, from the application of the poison, had entirely ceased, and the animal was apparently dead; but, on opening the thorax, the heart was found acting regularly eighty times in a minute, circulating dark coloured blood, and it continued to act for six or seven minutes afterward.

Experiment 6.

Exp. 6. I injected into the rectum of a cat half an ounce of water, with two drops of the essential oil. In two minutes afterward, he was affected with symptoms similar to those, which occurred in the last experiment; and at the end of five minutes, from the injection of the poison, he was apparently dead. Two minutes after apparent death, the heart was found acting eighty times in a minute. On dissection, no preternatural appearances were found either in the internal membrane of the rectum, or the brain.

Appears to act on the brain.

The symptoms produced by this poison, and the circumstance of the heart continuing to contract after apparent death, lead to the conclusion, that it occasions death by disturbing the functions of the brain.

Effects of a drop applied to the author's tongue.

While engaged in these last experiments, I dipped the blunt end of a probe into the essential oil, and applied it to my tongue, meaning to taste it; and having no suspicion, that so small a quantity could produce any of its specific effects on the nervous system; but scarcely had I applied it, when I experienced a very remarkable and unpleasant sensation, which I referred chiefly to the epigastric region, but the exact nature of which I cannot describe, because I know nothing precisely similar to it. At the same time there was a sense of weakness in my limbs, as if I had not

* The essential oil of bitter almonds does not appear to differ from the essential oil of laurel. I was furnished with a quantity of it, first by my friend Mr. William Brande, and afterward by Mr. Cooke of Southampton street.

the

the command of my muscles, and I thought that I was about to fall. However, these sensations were momentary, and I experienced no inconvenience whatever afterward.

I afterward applied a more minute quantity of the essential oil to my tongue several times, without experiencing from it any disagreeable effects; but on applying a larger quantity, I was affected with the same momentary sensations as in the former instance, and there was a recurrence of them in three or four seconds after the first attack had subsided.

Acts through the medium of the nerves.

From the instantaneousness, with which the effects are produced; and from its acting more speedily when applied to the tongue, than when injected into the intestine, though the latter presents a better absorbing surface; we may conclude, that this poison acts on the brain through the medium of the nerves, without being absorbed into the circulation.

Experiment with the Juice of the Leaves of Aconite.

Effects of the juice of aconite. Experiment 7.

Exp. 7. An ounce of this juice was injected into the rectum of a cat. Three minutes afterward he voided what appeared to be nearly the whole of the injection; he then stood for some minutes perfectly motionless, with his legs drawn together; at the end of nine minutes, from the time of the injection, he retched and vomited; then attempted to walk, but faltered and fell at every step, as if from giddiness. At the end of thirteen minutes, he lay on one side insensible, motionless, except some slight convulsive motions of the limbs. The respiration became slow and laboured; and at forty-seven minutes from the time of the injection, he was apparently dead. One minute and a half afterward, the heart was found contracting regularly one hundred times in a minute.

It acts in a similar way.

It appears from this experiment, that the juice of aconite, when injected into the intestine, occasions death by destroying the functions of the brain. From the analogy of other poisons, it is rendered probable, that it acts on the brain through the medium of the nerves, without being absorbed into the circulation. This opinion is confirmed by the following circumstance; if a small quantity of the leaf of aconite is chewed, it occasions a remarkable sense of numbness

Effects of chewing the leaf.

numbness of the lips and gums, which does not subside for two or three hours.

Experiments with the Infusion of Tobacco.

Effects of tobacco. Experiment 8. *Exp.* 8. Four ounces of infusion of tobacco were injected into the rectum of a dog. Four minutes afterward he retched, but did not vomit; he then became faint, and lay motionless on one side; at the end of nine minutes from the time of the injection, the heart could not be felt; he gasped for breath at long intervals: and in another minute there was no appearance whatever of life. I immediately laid open the cavities of the thorax and abdomen. The heart was much distended, and had entirely ceased to contract; there was no peristaltic motion of the intestines.

Experiment 9. *Exp.* 9. An ounce of very strong infusion of tobacco was injected into the rectum of a cat. Symptoms were produced similar to those, which occurred in the last experiment, and the animal died at the end of seven minutes from the time of the injection. On opening the thorax immediately after death, the heart was found extremely distended, and to have entirely ceased acting, with the exception of a slight tremulous motion of the auricles.

Exp. 10. *Exp.* 10. Three ounces of infusion of tobacco were injected into the rectum of a dog. He was affected with symptoms similar to those in the former experiments, and died at the end of ten minutes. On opening the thorax immediately after death, I found the heart much distended, and to have entirely ceased contracting.

Exp. 11. *Exp.* 11. Three ounces of infusion of tobacco were injected into the rectum of a dog. Immediately there took place tremulous contractions of the voluntary muscles. Five minutes afterward the injection was repeated in the same quantity. The dog then was sick, and threw up some of the infusion, with other matter, from the stomach; he became faint, and died ten minutes after the second injection. Immediately after respiration had ceased, I opened the thorax, and found the heart extremely distended, and without any evident contraction, except of the appendix of the right auricle, which every now and then contracted in a slight degree. I divided the pericardium on the right side. In

In consequence of the extreme distension of the heart, this could not be done without irritating the fibres with the point of the scalpel. Immediately both auricles and ventricles began to contract with considerable force, so as to restore the circulation. Artificial respiration was produced, and the circulation was kept up for more than half an hour, beyond which time the experiment was not continued.

We may conclude from these experiments, that the effect of the infusion of tobacco, when injected into the intestine of a living animal, is to destroy the action of the heart, stopping the circulation and producing syncope. It appeared to me, that the action of the heart ceased, even before the animal had ceased to respire; and this was confirmed by another experiment, in which, in a dog killed by the infusion of tobacco, I found the cavities of the left side of the heart to contain scarlet blood, while in those of the right side the blood was dark coloured. This poison therefore differs materially from alcohol, the essential oil of almonds, and the juice of aconite, which have no direct influence on the action of the heart. The infusion of tobacco renders the heart insensible to the stimulus of the blood, but it does not altogether destroy the power of muscular contraction, since the heart resumed its action in one instance on the division of the pericardium; and I have found, that the voluntary muscles of an animal killed by this poison are as readily stimulated to contract by the influence of the Voltaic battery, as if it had been killed in any other manner. At the same time, however, that the infusion of tobacco destroys the action of the heart, it appears to destroy also the functions of the brain, since these did not return in the last experiment; although the circulation was restored, and kept up by artificial respiration.

It destroys the action of the heart,

and also of the brain.

Since there is no direct communication between the intestinal canal and the heart, I was at first induced to suppose, that the latter becomes affected in consequence of the infusion being conveyed into the blood by absorption. Some circumstances in the following experiment have since led me to doubt, whether this is the case.

Its absorption by the blood questionable.

Exp. 12. In a dog, whose head was removed, I kept up the circulation by means of artificial respiration, in the manner

Exp. 12.

ner already described in the account of some experiments, which I lately communicated to this Society. I then injected into the stomach and intestines nine ounces of infusion of tobacco. At the time of the injection, the body of the animal lay perfectly quiet and motionless on the table; the heart acted regularly one hundred times in a minute. Ten minutes afterward the pulse rose to one hundred and forty in a minute; the peristaltic motion of the intestines was much increased, and the voluntary muscles in every part of the body were thrown into repeated and violent spasmodic action. The joints of the extremities were alternately bent and extended; the muscles of the spine, abdomen, and tail alternately relaxed and contracted, so as to turn the whole animal from one side to the other. I have observed, in other instances, spasmodic actions of the muscles, where the circulation was kept up by artificial respiration, after the removal of the head; but not at all to be compared, either in strength or frequency, with those, which took place on this occasion. I made pressure on the abdominal aorta for more than a minute, so as to obstruct the circulation of the blood in the lower extremities; but the muscular contractions were not lessened in consequence. Half an hour after the injection of the infusion, the artificial respiration was discontinued. The heart continued to act, circulating dark coloured blood; the muscular contractions continued, but gradually diminished in strength and frequency. I tied a ligature round the vessels at the base of the heart, so as to stop the circulation, nevertheless the muscular contractions still continued, though less frequent and forcible than before, and some minutes elapsed before they entirely ceased.

Remarks on the phenomena.

In this experiment, the disposition to contraction in the muscles was very much increased, instead of being diminished, as in those just related. If the infusion of tobacco influences the heart from being absorbed into the blood, and thus coming into actual contact with its fibres, there is no evident reason, why the removal of the brain, and the employment of artificial respiration, should occasion so material a difference in its effects. If the contractions of the voluntary muscles had depended on the infusion circulating with

with the blood, it is reasonable to suppose, that the pressure on the aorta would have occasioned some diminution of them, and that the complete obstruction of the circulation would have caused them to cease altogether.

From these considerations, I am induced, on the whole, to believe, that the infusion of tobacco, when injected into the intestines, influences the heart through the medium of the nervous system; but I have not been able to devise any experiment, by which the truth or fallacy of this opinion might be put beyond the reach of doubt. Appears to act on the nervous system,

It appears remarkable, that the brain and nervous system, although not necessary to the action of the heart, should, while under the influence of the infusion of tobacco, be capable of influencing this organ so as to stop its action; but this is analogous to what we see occur in consequence of violent emotions of the mind. Those states of the nervous system, which accompany the passions of joy, fear, or anger, when existing in a moderate degree, render the heart more sensible to the stimulus of the blood, and increase the frequency of its contractions; while, when the same passions exist in a greater degree, the heart is rendered altogether insensible to the stimulus of the blood, and syncope ensues. affecting it very powerfully.

*Experiments with the Empyreumatic Oil of Tobacco**.

Exp. 13. Less than a drop of this oil was applied to the tongue of a young cat. Instantly violent convulsions took place in all the muscles, and the respirations became very frequent. In five minutes after the application, she lay on one side insensible, with slight spasmodic actions of the muscles. At the end of eleven minutes, she retched, but did not vomit. In a quarter of an hour, she appeared to be recovering. I repeated the application of the poison, and she was again seized with violent convulsions, and became insensible, breathing at long intervals, and in two minutes from the second application respiration had entirely ceased, and she was apparently dead. On opening the thorax, I Effects of empyreumatic oil of tobacco. Exp. 13.

* I was furnished with the empyreumatic oil of tobacco by Mr. W. Brande. It may be procured by subjecting the leaves of tobacco to distillation in a heat above that of boiling water: a quantity of watery fluid comes over, on the surface of which is a thin film of unctuous substance.

 found

found the heart acting with regularity and strength, circulating dark-coloured blood. I introduced a tube into the trachea, and produced artificial respiration; the contractions of the heart became augmented in force and frequency, and there was no evident diminution in six or seven minutes, during which the artificial respiration was continued.

On dissection, nothing remarkable was found in the appearance of the tongue or brain.

Acted like the essential oil of almonds.

The symptoms and mode of death, in this experiment, did not essentially differ from those produced by the essential oil of almonds. I was surprised to find the effects of the empyreumatic oil so entirely different from those of the infusion of tobacco. Supposing that this difference might arise from the poison being more concentrated in the oil than in the infusion, I made the following experiments.

Exp. 14.

Exp. 14. A drop of the oil of tobacco was suspended in an ounce and a half of water by means of mucilage of gum arabic, and the whole was injected into the rectum of a dog. In two minutes afterward he became faint, retched, but did not vomit. He appeared to be recovering from this state, and in twenty-five minutes after the first injection, it was repeated in the same quantity. He was then seized with symptoms similar to those in the last experiment, and in two minutes and a half he was apparently dead.

Two minutes after apparent death, on the thorax being opened into, the heart was found acting regularly one hundred times in a minute, and it continued acting for several minutes.

Exp. 15.

Exp. 15. A drop of the empyreumatic oil of tobacco with an ounce of water was injected into the rectum of a cat. The symptoms produced were in essential circumstances similar to those, which occurred in the last experiment. The animal was apparently dead in five minutes after the injection, and the heart continued to contract for several minutes afterward.

We may conclude from these experiments, that the empyreumatic oil of tobacco, whether applied to the tongue, or injected into the intestine, does not stop the action of the heart and induce syncope, like the infusion of tobacco; but that it occasions death by destroying the functions of the brain

brain, without directly acting on the circulation. In other words, its effects are similar to those of alcohol, the juice of aconite, and the essential oil of almonds.

(To be concluded in our next.)

XII.

Analysis of a Chinese Gong-gong: by Mr. Klaproth*.

Among sonorous instruments the composition of copper with tin gives the loudest sound. Bells, we know, are composed of this alloy. The celebrated bell of Pekin, the largest in the World, which is twenty feet in diameter, and sixteen inches thick, is no doubt cast of it. Sonorousness of bell metal.

The Chinese frequently use another kind of bells too, which are not cast, but hammered out. These instruments, called *gongs*†, are not shaped like a common bell, but like a shield with the edge turned up: and give an astonishing sound when struck. Barrow, in his voyage to China, says of these instruments; that they are like flat pots, or rather potlids; that they are struck with a stick wrapped round with leather; and that they are supposed to be formed of copper, tin, and bismuth. The Chinese gong.

The thickness of this alloy is about that of the back of a knife; its colour is a bronze yellow; and its spec. grav. 8·815. Analysis of it.

A hundred and fifty grains were heated with nitric acid; and 42 grs of oxide of tin separated; answering to 33 grs of metallic tin.

Into the filtered liquor sulphuric acid was poured, and the mixture was evaporated to dryness. The residuum being dissolved in water, iron precipitated from it 117 grs of copper.

The gong therefore is composed of	Copper	78	Its composition.
	Tin	22	
		100.	

The property of emitting a sound that can be heard so far depends on the mutual penetration of the metals, and the greater density of the alloy, which is farther increased by hammering. Perhaps too the form of the instrument contributes to this. Cause of its loud sound.

* Ann de Chim. vol LXXV, p. 322.

† *Tshoung*, in the Chinese language, signifies a bell.

 XII.

XIII.

METEOROLOGICAL JOURNAL.

	Wind	Pressure. Max.	Min.	Med.	Temperature. Max.	Min	Med.	Evap.	Rain	
10th Mo.										
Oct. 9	S W	30·00	29·99	29·995	67	54	60·5	—		☽
10	S W	29·99	29·77	29·880	63	57	60·0	—		
11	S W	29·77	29·60	29·685	65	51	58·0	—		
12	S	29·80	29·60	29·700	62	48	55·0	·40	·08	
13	S W	29·90	29·86	29·880	62	49	55·5	—	·03	
14	S	29·86	29·81	29·835	63	51	57·0	—	·01	
15	S	29·76	29·75	29·755	73	53	63·0	·33		
16	S	30·03	29·76	29·895	70	55	62·5	—		
17	Var.	30·10	30·03	30·065	71	47	59·0	—		●
18	S W	30·16	30·10	30·130	68	50	59·0	—	·11	
19	Var.	30·21	30·18	30·195	65	49	57·0	·20		
20	S W	30·05	29·96	30·005	64	55	59·5	—		
21	S W	29·96	29·50	29·730	65	56	60·5	·20		
22	S	29·52	29·46	29·490	64	50	57·0	—	·15	
23	S W	29·50	29·48	29·490	60	49	54·5	—		
24	Var.	29·48	29·35	29·415	57	42	49·5	·20	·08	
25	S	29·35	28·65	29·000	53	38	45·5	—	·18	☾
26	Var.	28·80	28·65	28·725	54	41	47·5	·15	·32	
27	S E	28·84	28·81	28·825	56	43	49·5	—	·11	
28	Var.	28·84	28·80	28·820	56	41	48·5	·15	·44	
29	S W	29·05	29·00	29·025	55	43	49·0	·02	.18	
30	Var.	29·55	29·00	29·275	58	43	50·5	—	·14	
31	W	29·77	29·68	29·725	59	48	53·5	·15	·14	○
11th Mo.										
Nov. 1	S W	29·68	29·62	29·650	62	57	59·5	—	·11	
2	S W	29·58	29·50	29·540	62	53	57·5	·34	·14	
3	S W	29·70	29·60	29·650	58	48	53·0	—	·08	
4	W	29·98	29·80	29·890	60	42	51·0	·17	—	
5	S W	29·89	29·83	29·860	56	43	49·5	—	·25	
6	S W	29·83	29·52	29·675	53	45	49·0	·13	·50	
		30·21	28·65	29·614	73	38	54·86	2·44	3·05	

N. B. The observations in each line of the Table apply to a period of twenty-four hours, beginning at 9 A. M. on the day indicated in the first column. A dash denotes, that the result is included in the next following observation.

NOTES.

NOTES.

Tenth Month. 12. Windy: wet evening. 13. Much wind. 14. A shower before nine a. m. at which time occurred the max. of temp. 15 Much dew on the grass: serene day: twilight milky, with converging streaks of red. 16. a m. Much dew: a mist on the river: the smoke of the city remarkably depressed, and sounds unusually strong from thence: some thunder clouds appeared and passed to E. 17. *Cumulus* clouds surmounted with *cirrostratus*, and *cirri* above. 18. A very wet mist a. m. wind N.W.: at two p. m. cloudy; very moist air, the *dew point* (or temperature at which a body colder than the air condenses water from it) being 63°: about sunset, at temp. 63°, I found dew just beginning to be deposited on the grass: it rained hard about five next morning. 19. a. m. Misty, small rain: p. m clear: evening, *cirri* very elevated, and long coloured red; a *stratus* forming. 20. Misty: then overcast: the wind, which had been E., veering by S.: abundance of *gossamer.* A quicken-tree (*sorbus aucuparia*) exhibits a new set of leaves and blossoms along with the ripe berries: 21. Gray morning, with little dew and a strong breeze. 22. a. m. Dew scarce perceptible: wind veers to S., a breeze: p. m. very cloudy, with showers: much wind at night. 24. At mid day a drizzling rain, during which the vane turned to E. 25. Clear, fine day: wind veered to S.: at sunset *nimbi* and *cirrostrati* in S. W.: heavy shower by eleven p. m. 26. Showery: a fine rainbow at ten a. m. 27. a. m. *Nimbi* in different quarters, mixed with *cumulus* and *cirrostratus*, beneath large plumose *cirrus* clouds. 28. a. m. Clear, much dew, *nimbi* forming amidst various clouds: vane at N. E.: p. m. a shower in the S., during which appeared, for a short time, a numerous flight of swallows: they had been last observed on the 15th: the wind returned by S. to N. W. with much cloud and rain. 30. At nine a. m. the rain intermitting, the highest and most considerable mass of clouds was moving from W. an intermediate portion from S., and the wind below fresh at E.: in this state of things sounds came very freely from the westward, and by eleven the wind was S. W.: at three p. m. distinct *nimbi* and a bright bow: showery at night, with a lunar halo. 31. a. m. Clear: the sun and moon appeared red on the horizon: at night, the wind being S. sounds came loud from the W.

Eleventh Month. 1. a. m. Much cloud: wind fresh at S. W. 2. As yesterday: stormy at night 3. A rainbow at eight a. m. 4. a. m. *Nimbi* to windward: at sunset, the dense clouds in the E. finely coloured: rainbow: wind W. 5. a. m. Stormy: p. m. wet. 6. Cloudy, showery: evening, abundance of *cirrostratus*: a wet night.

RESULTS.

Barometer: highest observation 30·21 inches; lowest 28·65 inches; range 1·56 inches.
Mean of the period 29·614 inches.

Thermometer: highest observation 73°; lowest 38°; range 35°.
Mean of the period 54·86°.

Evaporation 2·44 inches. Rain 3·05 inches.

Wind with little exception S. W. and S. The fore part of the period changeable; the latter wet, without the usual intervening frosty nights.

L. HOWARD.

PLAISTOW, *Eleventh Mo.* 20, 1811.

XIV.

Chemical Examination of the yellow Resin of the Xanthorrhœa hastilis, and of the resinous Cement employed by the Savages of New Holland to fix the Stone of their Hatchets: by Mr. A. Laugier.*

Yellow gum from Botany Bay.

THE following remarks on the resin of the xanthorrhœa, and the tree that produces it, which Mr. Peron has been so obliging as to communicate to me, will form a very suitable introduction to the experiments I shall relate, and enhance their value.

The tree.

"The resin in question," says Mr. Peron, "exudes naturally from the bark of a tree peculiar to New Holland, and of which Dr. Smith has made a new genus, under the name of *xanthorrhœa hastilis*; thus intending to express in one term the colour of the resin of this strange tree, and in the other the use, which the natives make of its shoots for their spears.

Its name not unexceptionable.

"It must be observed however, that Dr. Smith's generic name is not strictly accurate; as the resin is very frequently brown, red like dragon's blood, green, &c. Hence the different names of yellow, red, green, &c., gum plant, or gum tree, given almost indiscriminately to the xanthorrhœa by the English at Port Jackson. Whether these varieties of colour indicate so many species, or varieties, of the tree that produces them; or depend merely on the age or other circumstances of the individual tree; has not yet been ascertained.

Probably several species.

"Hitherto botanists have admitted only one species of xanthorrhœa, the hastilis, just mentioned: but as trees of this kind are found throughout the various parts of New Holland, an extent of country equal to all Europe, it is very probable, that several species exist.

Phillip's description and figure indifferent.

"Governor Phillip, in his voyage to Botany Bay, p. 60, and plate to p. 119, has given an incomplete description of the xanthorrhœa; and a figure, which, though not very carefully executed, is sufficient to afford an idea of this extraordinary tree.

* Ann. de Chim. vol. LXXVI, p 265.

"It

"It is particularly abundant at Geographer's bay, Leuwin's Land, and in the environs of Botany Bay; and appears to prefer a sandy and barren soil. The shoots, which the savages use for their spears, extend to the length of three, four, or even five yards; and are nearly of the same size, which is scarcely equal to that of the thumb, throughout their whole length. Its soil, and growth.

"Each of these shoots terminates in a kind of spike, or ear, of a larger size, and from fifteen to twenty four inches long; from the surface of which exudes a kind of viscous liquid, of a pleasant saccharine taste, and a strong aromatic smell. The savages are very fond of it: and I found it, on tasting it, to be as I have described. To procure these tops of the xanthorrhœa, the natives have recourse to their clubs [*casse-tête*], which they throw with such strength and skill, that they are sure to cut off the ear at what length they please at the first stroke. Produces a sweet juice.

"The resin flows naturally from the trunk of the tree, making its way through the bark. The portion of the stem, that is buried in the sand, appears to furnish the greater part; at least large pieces are found in the sand, apparently still adhering to the bark. Some of these pieces are remarkable for the perfect regularity of their spherical form. The resin.

"The English employ this resin against dysentery, for which they esteem it an excellent medicine. The savages use it for many domestic purposes, and particularly for cementing the points of their spears to the shaft. With this substance too they prepare the celebrated instrument, that serves to discharge their spears; also their fishing implements, their stone hatchets, &c. They likewise employ it to unite the lips of wounds, however large or dangerous they may be; and I have seen some healed in this way by the first intention, that have appeared to me truly extraordinary. Its uses.

"The wood of the xanthorrhœa, when burned, emits a smell which is very pleasant, at a little distance from the fire, but seemed to me too powerful if inhaled nearer. Such indeed is the odoriferous strength of this wood, that you may sometimes discover a party of savages more than a quarter The wood fragrant when burned.

quarter of a league [half a mile] distant merely by the smell it emits in burning.

Probably the eagle wood of India.

"Mr. Martin-Moncan, formerly agent of the French government to Hyder Ali Khan, told me, on seeing a piece of the xanthorrhœa, and smelling to it, that it very much resembled the celebrated eagle wood, which fetches such a high price in India, and the country of which is hitherto unknown to Europeans. Mr. Martin-Moncan considered it as by no means impossible, that the Malays, who in fact have long had a commercial intercourse with New Holland, visit its coast to procure the wood of the xanthorrhœa, which he believes to be the eagle wood itself."

Physical properties of the yellow resin.

The resin of the xanthorrhœa is friable and easily separates into scales before the nail. Its fracture is shining and compact. It has a yellow colour, and a very pleasant balsamic smell, resembling that of poplar buds. When rubbed in a mortar, it clots, and adheres to it strongly. It is rendered very perceptibly electric by friction. The paper on which it has been put when powdered retains enough of it to acquire a deep yellow colour, which cannot be removed.

Action of heat on it.

Exposed to a gentle heat, it melts, swells up, gives out a considerable portion of aqueous vapour, diminishes in bulk, and acquires a brownish red colour inclining to purple. Placed on burning coals, it rises in dense fumes, very pungent, and so strongly aromatic as to be disagreeable; and soon after it flames, swells up considerably, and leaves a very bulky and very light coally residuum.

Action of alcohol.

As this substance does not mix with water, and imparts to it no colour, acting in this respect as a resin, I employed for analysing it alcohol at 40° [sp. grav. 0·817], which dissolved it with the greatest facility, and without the assistance of heat. Nothing was left undissolved but 0·07 of an insipid, grumous substance, resembling a gum, and particularly that which is called in the shops gum of Bassora, for it is neither soluble nor diffusible in water, it is only softened and swelled up by the action of this fluid when boiling.

The tincture partly precipitated by water.

The alcoholic solution when filtered has a reddish colour, and is remarkable for its limpidity and pleasant smell. It may be kept several months without undergoing any alteration.

tion. Water renders it turbid, and occasions a precipitate, but a portion of the resin remains suspended, without being separable either by heat or standing; so that the mixture resembles a solution of gum-resin. If however it be heated long enough to evaporate the alcohol, and about three fourths of the liquid, almost all the resin is deposited on the bottom and sides of the vessel, and the portion more minutely divided unites on cooling into little tufts of a lemon colour. The mixture in this state has a more pleasant and delicate smell than the resin itself, and some compare it to that of storax.

Benzoic acid obtained from the water.

The water separated from the resin was still turbid, or a little coloured, and reddened vegetable blues. In order to fix the acid it contained, I had recourse to the process, which I employed with success in my analysis of the substance found in the grotto of Arc, and of castor, to obtain from it the benzoic acid: I added a few drops of caustic potash, and I evaporated to dryness. The residuum, which resembled a kind of brown red extract, was distilled with a little sulphuric acid diluted with water, and toward the end of the process I obtained a few small crystals, which had the characteristics of benzoic acid.

These small crystals I diffused in the acid and aromatic water in the receiver, and supersaturated the mixture with lime quenched in the air. After evaporating to dryness, I poured on the residuum a small quantity of cold water, to take up the benzoate of lime, and separate it from the sulphate and carbonate of this base, which were mingled with it. Into the filtered and concentrated liquor I poured muriatic acid, which produced in it a slight precipitate of benzoic acid in the form of small granular crystals.

Readier mode of obtaining this acid.

But I found, that the most simple and ready mode of distinguishing the presence of this acid in the yellow resin was, to expose this substance to a heat sufficient to keep it in fusion. I introduced the powdered resin into a very dry vessel, which I placed on a sand-heat: and as soon as the resin was melted aqueous vapours first rose, and soon after white fumes, which condensed on the sides in small shining scales, exhibiting all the characteristics of benzoic acid.

As the acid is expelled, the resin first swells up: after

which

which it collapses, and diminishes in bulk. In this state it is of a deep brown colour, which appears purplish when placed between the eye and the light.

The alcoholic solution also yields a few crystals of benzoic acid by distillation to dryness, though not so easily. The alcohol distilled from it reddens litmus paper, which shows, that it probably carries off a portion of the same acid.

The resin distilled with water yielded a hot and fragrant oil.

I introduced 30 grains of the yellow resin into a retort with four ounces of distilled water, fitted to it a receiver, and distilled on a sand-heat. The water, that passed into the receiver, was turbid, on account of the suspension of a certain quantity of essential oil, several drops of which collected on the surface. The water thus mixed with oil had an extremely pleasant smell. The extremity of the beak of the retort was soiled with this oil, which had an acrid and burning taste nearly like that of the oil of cloves. When the matter remaining in the retort was dry, white fumes arose, that condensed in the head of the retort in small and very white crystals, which powerfully reddened litmus paper, and had the strong and pleasant smell of benzoic acid.

This oil obtained from the tincture.

The essential oil of the yellow resin may be obtained also by distilling the alcoholic solution: the alcohol, that passes over into the receiver, being insensibly impregnated with it; so that the evaporation of this liquid by a gentle heat is sufficient to procure this acrid and pleasant substance.

The resin forms a soap with alkalis or lime.

Caustic alkalis, or lime, placed in contact with the yellow resin, immediately assume a deep yellow colour, without the assistance of heat; and dissolve the resin completely, if they be employed in sufficient quantity. The solution froths when shaken, like that of soap, and lets fall a yellowish white precipitate on the addition of an acid.

The benzoic acid not obtainable from this.

I had hoped, that this solvent action of the alkalis would furnish me with an easy mode of separating the benzoic acid from the resin; but several trials convinced me of the impossibility of succeeding. It appears, that this acid falls down at the same time as the resin, the moment another acid is added to the mixture.

The resin

Thirty grains of the yellow resin in powder being heated in a retort with six times the weight of nitric acid, a considerable

derable evolution of nitrous gas was produced, and the resin was completely dissolved. The liquor remaining in the retort deposited by cooling a crystalline substance; and both the mother-water and the crystals were of a deep yellow colour, a very bitter taste, and a smell of bitter almonds. A portion of the mother-water being saturated with potash, it did not emit any sensible ammoniacal smell; but being mixed with a solution of sulphate of iron, and supersaturated with concentrated sulphuric acid, it let fall in the course of the night a considerable quantity of Prussian blue. Another portion of the same mother-water yielded on evaporation thin crystals several lines square, which might be known for oxalic acid. Their solution precipitated lime-water, and the calcareous salts. treated with nitric acid.

From the experiments I have related it appears, that the yellow substance, which flows from the xanthorrhœa, is composed of a large portion of resin, combined with a few hundredths of a kind of spongy gum, insoluble in water, of benzoic acid, and of a very acrid, yellowish volatile oil, very pleasant to the smell. Conclusions.

The yellowish substance of the xanthorrhœa then cannot be considered as a resin, properly so called; since it differs from resins in containing benzoic acid, to which it is indebted at least in some measure for its pleasant smell; and on this account it seems to belong rather to the balsams, than to the resins. It is properly a balsam.

What struck me most in the examination of this yellow substance is its resemblance to that matter, which the bees employ for stopping cracks in their hives, and to which the name of propolis has been given. Resembles propolis.

This resinous, odoriferous matter, when separated from the wax, by which its properties are concealed, exhibits the characters of the yellow substance; and, if subjected to the same processes, comports itself in the same manner.

It is considered by naturalists as ascertained almost to a demonstration, that the resinous matter, which covers the buds of poplar trees, and preserves them from moisture, is that which the bees so carefully collect, to form their propolis. The smell of this matter, which is precisely the same with that of the propolis, strongly supports this opinion. The resin on poplar buds similar.

The

The smell of the yellow substance too is similar to that of the poplar buds: and, if we cannot hence infer its perfect identity with propolis, it is at least certain, that the difference between them is too trifling to admit the supposition, that bees could not employ the yellow substance for the same purpose. This conjecture, however, might easily be verified in countries where the tree that produces it so abundantly grows.

A cement of great strength made with this resin.

The resin I have just analysed enters into the composition of a cement, which the natives of New Holland employ for fixing the stone of their hatchets to the handle, and for securing the points of their spears. This cement is capable of acquiring such hardness, that the hardest substances cannot separate it, or even loosen the stone fastened by it. Its colour is a deep brown; and on rubbing it emits a fragrant smell, which does not differ from that of the yellow resin.

This cement analysed.

I satisfied myself of the complete identity of this cement with the yellow resin by examining a sufficient quantity of it, taken from a hatchet brought home by Mr. Peron, and which her majesty, the empress Josephine, deigned to accept from that navigator, as a valuable proof of the industry of the natives of Nuyts's Land.

A hundred parts of the brown powder furnished by the cement were digested in alcohol at 40° [sp. gr. 0·817]. Two portions of this liquid added in succession were sufficient to take up all the resin, that the cement contained. What remained after the action of the alcohol was nothing but a blackish gray powder, without smell or taste. The weight of this residiuum was 51 parts, so that the alcohol had taken up 49.

The alcoholic solution had a deep red colour, and was exactly similar to that obtained by macerating in the same menstruum the yellow resin, after it had been melted and turned brown by heat. On evaporation it yielded a red resin, which had all the characters of the resin of the xanthorrhœa.

On the 51 parts not dissolved by the alcohol I boiled to dryness a small quantity of nitric acid, which caused the residuum to acquire a redness like that of oxide of iron, and I treated this residuum with muriatic acid. After the action

tion of this acid, the residuum, being 37 parts, was a white, dry powder, rough to the finger, and resembling fine sand.

Ammonia, poured into the muriatic solution, separated 7 parts of oxide of iron; and oxalate of ammonia produced a precipitate equivalent to 3 parts of lime.

This chemical examination shows, that 100 parts of the resinous cement are formed of

Its component parts.

Yellow resin	49
Pure sand	37
Oxide of iron	7
Lime	3
Loss	4
	100

It appears, that necessity has taught the natives of New Holland a practice, which engravers employ every day. It has taught them, to mix a proper quantity of sand with the yellow resin kept some time in fusion, and thus to compose a cement capable of acquiring considerable hardness.

Similar to engraver's wax,

This is the mode in which the resinous cement, called in the shops engravers' wax, is prepared. Brickdust is added to common resin: the mixture is melted, and cast in moulds: and thus it is formed into red cakes, which are sold to the engravers. I have satisfied myself, that, the oftener this mixture has been melted, the harder it is.

I examined engravers' wax in comparison with the cement of the savages of New Holland; and I observed with surprise, that the proportions of resin and brickdust were precisely the same with those of the yellow resin and sand in the cement I analysed.

but harder.

It appeared to me, however, that the engravers' wax, though very hard, particularly when it has been melted several times, is inferior in solidity to the cement of the natives of New Holland; a difference that may be ascribed to the difference between the resins, and the greater or less force, with which their particles cohere.

XV.

XV.

Note on the Precipitation of Silver by Copper: by Mr. GAY-LUSSAC*.

Silver precipitated from its solution by copper impure:

MOST chemists are of opinion, that the precipitate, obtained by leaving a slip of copper in a solution of nitrate of silver, is an alloy of the two metals, and that consequently it is impossible to procure pure silver by this process. This is the truth of the fact, when no attention is paid to particular circumstances: but if we examine the different stages of the precipitation, and attend to the causes that produce them, we shall soon perceive, that it is easy to obtain silver free from the copper by which it is precipitated.

but may be obtained otherwise.

The first portions separated pure:

In fact, the first portions of silver separated are commonly pure, and do not give a blue tinge to ammonia, when they are dissolved in nitric acid. It is only in proportion as the copper enters into solution, that we find any in the precipitate; so that toward the end of the process the quantity becomes very evident. If therefore we separate the first portions of silver, we shall find them exempt from copper: but, to obtain considerable quantities, we may take the whole of the precipitated silver, as I have done, wash it, and digest it with a small quantity of nitrate of silver: by these means, the copper will be redissolved, and a corresponding quantity of silver precipitated.

and the whole may be rendered so by adding nitrate of silver.

An affinity between the metals may occasion an alloy to fall down, but not in this case.

I am far from thinking, that the mutual action of metals is incapable of occasioning the formation of alloys in metallic precipitations: I only conclude, that, in the experiment I have just related, the precipitation of the copper is not occasioned by the affinity between this metal and silver; since in this case we ought to have the same alloy in every stage of the precipitation, and besides this could not be destroyed by being placed in contact with a fresh quantity of nitrate of silver. Precipitation in general being the effect of a galvanic process: it appears to me, that the copper, which is reduced by hidrogen as well as silver, is precipitated with this metal by the same cause. Many other metallic precipitations would exhibit similar results.

Galvanism acts in the precipitation.

* Annales de Chim. vol. LXXVIII, p. 91.

XVI.

XVI.

Table expressing the Quantities of Sulphuric Acid at 66° [spec. grav. 1·842] contained in Mixtures of this Acid and Water at different Degrees of the Areometer; by Mr. VAUQUELIN*.

THE use that is made at present of sulphuric acid of different strengths for various uses, and particularly for the manufacture of soda, has rendered it necessary for the manufacturers and consumers of this acid, to inquire into the quantity of concentrated acid, that is, at 66°, indicated by the different degrees of the areometer. Strength of sulphuric acid an object of inquiry to manufacturers.

Concentrated sulphuric acid not being necessary for the decomposition of muriate of soda, that which is carried to 50° in the chamber being even preferable, both the manufacturer and consumer would find their advantage in the use of this. But to settle the price of this acid, according to the various degrees marked by the areometer, we must know how much acid of 66° there is at each degree, which can be found only by experiment; the quantities of acid not being in the direct ratio of the degrees, in consequence of the condensation that takes place on the combination of the acid with water. Best strength for decomposing sulphuric acid.

Having been very frequently consulted on this subject, I have thought it would be useful, to construct a table by means of experiments, in which the degrees of the areometer should show the weights of acid at 66°. A table of strengths useful.

For this purpose I began with taking accurately the specific gravity of the sulphuric acid at 66°, which I used in making my mixtures; and I found it to be 1·842, distilled water being taken as the unit, at the temperature of 12° of Reaum. [59° F.]. I then sought the quantities of this acid and water necessary to produce the degrees of the areometer used in trade to measure the density of this acid, beginning at 60°, and proceeding downward by fives till I came to 5°. Method in which the following was constructed.

The weights were ascertained with great care by means of a very sensible balance: the vessel, in which I made my mixtures, was constructed so that the vapours formed by

* Annal. de Chim. vol. LXXVI, p 260.

the

the heat evolved in each instance could not escape; and I was careful not to take the degree on the areometer, till the mixture had returned to 12° R. [59° F.].

I reduced to hundredth parts the quantities of water required to obtain the degrees on the areometer, which necessarily gave me fractions.

It may be objected, that the intervals in my table are too great; and I confess it would have been better, to make as many mixtures as the concentrated acid marks degrees on the areometer, namely 66: but, not to mention that this would have rendered my undertaking tedious and difficult, it would not have been of any great use for the purposes of trade, for which it was chiefly intended. In fact, the quantity of acid in any degree in these intervals may be obtained very nearly by means of a simple sum in the rule of proportion.

Lastly, I have taken the specific gravity of each of my mixtures, which will give the means of ascertaining the quantity of acid and water in such mixtures, when an areometer is not at hand. These specific gravities too will show the degree of condensation, that water experiences, in combining with sulphuric acid in the different proportions employed.

Table of mixtures of sulphuric acid and water.

Deg. of Areom.	Specif. gravity.	Sulph. acid at 66°.	Water.
5	1·023	6·60	93·40
10	1·076	11·73	88·27
15	1·114	17·39	82·61
20	1·162	24·01	75·99
25	1·210	30·12	69·88
30	1·260	36·52	63·48
35	1·315	43·21	56·79
40	1·375	50·41	49·59
45	1·466	58·02	41·98
50	1·524	66·45	33·55
55	1·618	74·32	25·68
60	1·725	84·22	15·78
66	1·842		

A
JOURNAL
OF
NATURAL PHILOSOPHY, CHEMISTRY,
AND
THE ARTS.

SUPPLEMENT TO VOL. XXX.

ARTICLE I.

On the Place of a Sound, produced by a musical String. In a Letter from Mr. JOHN GOUGH.

To. MR. NICHOLSON.

SIR,

CERTAIN experiments and remarks of mine on the augmentation of sounds appeared in the tenth volume of your Journal; the intention of which communication was to show, that the range of a sound may be greatly extended, by enlarging the vibrating surface, while the magnitude of the impulse remains the same. Among other remarks contained in that paper, a fact is mentioned; which proves, that the audible effect of a musical string varies with the texture of the instrument to which it is attached; or, to use the language of certain writers on acoustics, the force of such a string depends not a little on the conducting power of the frame upon which it is stretched. Former observations referred to.

Perhaps this assertion will be called a novelty in the theory of stringed instruments; for I believe, that the philosophers, who have turned their thoughts to the subject, are unanimous in maintaining, that the effect, which a vibrating fibre produces on the ear, proceeds solely from the pulses, excited in the air by the undulatory motion of the cord. In consequence of this doctrine, they make a musical string to be the seat of the sound which it occasions, Remarks on the common theory of musical strings.

in the same manner as a bell, a drum, and a tambarine may be called the seats of the sounds, which they impart to the ear through the medium of the atmosphere. Though I do not deny, that pulses are produced in the air by slender fibres in the act of vibration, I have long disputed the accuracy of the prevailing theory, without being able to demonstrate the truth of the suspicion to my own satisfaction. An accidental observation, however, attracted my notice lately, which proves the string to be the exciting cause; and shows, that the sound proceeds from the frame or body of the instrument, in the same manner that the sound of a bell proceeds from that vessel. The circumstance here alluded to suggested the following easy experiments; which any one may repeat, who wishes to be convinced of the fact by his own experience.

Two experiments contradicting the received theory.

Exp. 1. One end of an iron wire (No. 28) was fastened to a brass knob screwed into a table of deal, and the other end was wrapped round a slender cylinder of yew; four or five inches long. The wire measured six feet betwixt the knob and cylinder, and I stretched it with considerable force, by holding the wooden pin in my hand, so as to let no part of the string touch my fingers. The wire being then made to vibrate, the sound, produced on the occasion, came from the table; not only in my opinion, but also in the judgment of several persons, before whom the experiment has been repeated at different times.

Exp. 2. If, in stretching the wire, one end of the yew cylinder was made to press upon a second table, placed five feet from that into which the brass knob was fixed, the surface became the seat of sound, that supported my hand and the wooden pin. But when the cylinder was removed to a small distance from the table, on which it pressed, and the wire was kept stretched at the same time, the sound was heard instantaneously as in the first experiment proceeding from the opposite table. It seems adviseable to remind the reader of a well known fact, before the inferences are stated, which appear to be deducible from the preceding experiments. When a number of sounds strike the ear at the same time, one of which is much more powerful than any of the rest; all the weaker escape notice, and the seat of the strongest is alone recognised: in more familiar language

A well known fact stated.

guage it is the only one of the number, which the observer hears.

Now three sets of vibrations are evidently going on at the same time, in the first experiment; these are the primary set of the wire, and the two derivative sets existing in the table at one end of it, and in the wooden pin at the other. But the length of the string enables the ear to ascertain which one of the three sets gives the seat of the sound; and this is the vibratory motion of the table; consequently the table is the sounding body, and the wire does nothing more than perform the part of a drumstick in causing the surface of it to vibrate with great celerity. This discovery points out a distant analogy connecting the thundering noise of a drum and the smooth sounds of a harp or lute. They are, however, very distinct to sense for obvious reasons: the cover of the former instrument is highly elastic, and the sound of it continues to die away for some seconds after it has been struck; each stroke of the drumstick renews this sound, and the interval between two succeeding strokes is sufficiently large to be observed by the ear; and hence proceeds the thundering noise of a drum. On the contrary, the sound derived from the wooden frame of a stringed instrument by a single stroke is very transient; but the impulses of the strings beat upon it with a celerity, which does not permit the sound to suffer a sensible diminution of force in the interval of two successive strokes; which is the cause of smoothness in tones of this sort.

A vibrating string performs the part of a drumstick on a harpsichord, violin, &c.

In the second experiment, the vibrations, communicated from the wire to the cylinder of yew, are imparted by contact to the other table, which thereby becomes the seat of sound; because, being nearer the person of the experimenter, it makes a more powerful impression on his ear, than the first table; which stands at a greater distance from him. But so soon as the cylinder ceases to touch the board, that supports it, the experimenter hears the sound from the opposite table; notwithstanding it is farther from his ear than the wooden pin in his hand. Hence we discover the utility, and even the necessity of extensive surfaces in the production of sounds; for the impulses received from the vibrating wire, by the table and cylinder, are equal in number and magnitude; but of the three sets of contemporary vibrations, that existing in the table is alone heard.

The necessity of extensive surfaces in the production of sound.

 The

The frame of a stringed instrument is the seat of sound.

The foregoing facts and observations demonstrate, that the pulses excited in the air by a vibrating cord do not make any sensible impression on the organs of hearing; on the contrary, the sound, which we attribute to a musical string, comes in reality from the frame, upon which it is stretched. This errour of judgment arises from the proximity of the cord and frame, which prevents the ear from determining whether of the two is the sonorous body; we therefore ascribe the sound to the part that sustains the impulse. It is true, indeed, that the notes of a harpsichord or violin are caused by the vibrations of the strings; but then the various modifications, incident to these rapid and delicate motions, are imparted to the ear through the medium of the less elastic frame; the momentary sounds of which change their character when acted upon by a quick succession of impulses, and become continuous.

Middleshaw, Dec. 6, 1811. JOHN GOUGH.

II.

Experiments and Observations on the different Modes in which Death is produced by certain vegetable Poisons; By B. C. Brodie, *Esq. F. R. S. Communicated by the Society for promoting the Knowledge of Animal Chemistry.*

(*Concluded from p.* 307.)

Poisons applied to wounds.

III. *Experiments with Poisons applied to wounded Surfaces. Experiments with the essential Oil of Almonds.*

Essential oil of almonds. Experiment 16.

Exp. 16. I MADE an incision in the thigh of a rabbit, and introduced two drops of essential oil between the skin and the muscles. In four minutes after the application, he was seized with violent convulsions, and became insensible, and in two minutes more he was apparently dead; but the heart was felt through the ribs acting one hundred and twenty times in a minute, and it continued acting for several minutes. There were no other appearances in the limb, than would have resulted from an ordinary wound.

Experiment 17.

Exp. 17. Two drops of the essential oil of almonds were introduced into a wound in the side of a mouse. Two minutes afterward he was affected with symptoms similar to those

those which occurred in the last experiment, and in two minutes more he was apparently dead; but the heart continued to contract for some minutes afterward.

From the experiments which I have just related, and from others which it appears unnecessary to detail, as the general results were the same, I have learned, that, where the essential oil of almonds is applied to a wound, its effects are not so instantaneous as when it is applied to the tongue; otherwise there is no difference in its effects, in whatever manner it is applied.

Acts as when applied to the tongue, but less quickly.

Experiment with the Juice of the Leaves of Aconite.

Exp. 18. I made a wound in the side of a young rabbit, and introduced between the skin and muscles about twenty drops of the juice of aconite. Twenty-three minutes afterward he was affected with symptoms in all essential respects similar to those, which occurred in an experiment already related, where the juice was injected into the rectum; and at the end of forty-seven minutes from the application of the poison, he was apparently dead. Two minutes after apparent death, the heart was found contracting, but very feebly.

Juice of aconite Experiment 18.

Experiments with the Woorara.*

Exp. 19. A small quantity of the woorara in powder was applied to a wound in the side of a guinea pig. Ten minutes afterward the animal was unable to walk; then he became quite motionless, except some slight occasional convulsions. He gradually became insensible, the respirations were laboured, and at the end of fourteen minutes from the application of the poison, the respiration had entirely ceased, and he was apparently dead; but on opening the thorax, the heart was found acting seventy times in a mi-

Woorara. Experiment 19.

* The woorara is a poison, with which the Indians of Guiana arm the points of their arrows. It appears not to differ essentially from the ticunas, which was employed in the experiments of the Abbè Fontana. I am indebted to Dr. E. N. Bancroft, who not only furnished me with some of the woorara, which he had in his possession, but also lent me his assistance in the experiments, which were made with it.

nute,

nute, circulating dark coloured blood, and it continued to contract for several minutes afterward. On dissection no preternatural appearances were observed in the brain; nor was there any other appearance in the limb, than would have arisen from an ordinary wound.

Experiment 20. *Exp.* 20. I made a wound in the side of a guinea pig, and introduced into it about two grains of the woorara in powder. At the end of twenty-five minutes, symptoms took place very similar to those, which occurred in the last experiment, and in thirteen minutes more the animal was apparently dead; but the heart continued to contract one hundred and eight times in a minute, and by means of artficial respiration the circulation was kept up for more than twenty minutes.

It acts on the brain. The results of other experiments, which I have made with the woorara, were similar to those just described. The heart continued to act after apparent death, and the circulation might be kept up by means of artificial respiration. It is evident, that this poison acts in some way or other on the brain, and that the cessation of the functions of this organ is the immediate cause of death.

Best applied dissolved in water. I found in these experiments, that the best mode of applying the woorara is when it is dissolved in water to the consistence of a thin paste. I first made the wound, and then smeared the poison over it with the end of the scalpel. I found that the animal was more speedily and certainly affected, if there was some hæmorrhage; unless the hæmorrhage was very copious, when it produced an opposite effect, by washing the poison away from the wound. When the poison was applied in large quantity, it sometimes began to act in six or seven minutes. Never more than half an hour elapsed from the time of the poison being inserted, to that of the animal being affected, except in one instance, where a ligature was applied on the limb, which will be mentioned afterward. Probably weak from age. The woorara, which I employed, had been preserved for some years, which will account for its having been less active, than it has been described to be by those, who had witnessed its effects when in a recent state.

Experiments

Experiments with the Upas Antiar *.

Exp. 21. About two grains of this poison were made into a thin paste with water, and inserted into a wound in the thigh of a dog. Twelve minutes afterward he became languid; at the end of fifteen minutes, the heart was found to beat very irregularly, and with frequent intermissions; after this, he had a slight rigour. At the end of twenty minutes, the heart beat very feebly and irregularly; he was languid; was sick and vomited; but the respirations were as frequent and as full as under natural circumstances, and he was perfectly sensible. At the end of twenty minutes, he suddenly fell on one side, and was apparently dead. I immediately opened into the thorax, and found the heart distended with blood in a very remarkable degree, and to have entirely ceased contracting. There was one distinct and full inspiration, after I had begun making the incision into the thorax. The cavities of the left side of the heart contained scarlet blood, and those of the right side contained dark coloured blood, as in a living animal. Upas antiar. Experiment 21.

Exp. 22. A small quantity of the upas antiar, prepared as before, was inserted into a wound in the thigh of a young cat. She appeared languid in two minutes after the poison was inserted. The symptoms, which took place, did not essentially differ from those, which occurred in the last experiments, except that there were some convulsive motions of the limbs. At eight minutes after the poison was inserted, she lay on one side, motionless and insensible; the heart could not be felt, but the respiration had not entirely ceased. On opening into the thorax, I found the heart to have ceased contracting. It was much distended with blood: and the blood in the cavities of the left side was of a scarlet colour. There were two full inspirations after the incision of the thorax was begun. On irritating the Experiment 22.

* We are informed, that the island of Java produces two powerful vegetable poisons, to one of which the natives give the name of *upas tieutè*, and to the other that of *upas antiar*. I was supplied with a quantity of the latter through the kindness of Mr. Marsden, who had some of it in his possession.

the heart with the point of the scalpel, slight contractions took place in the fibres of the appendices of the auricles, but none in any other part.

Experiment 23. *Exp.* 23. The experiment was repeated on a rabbit. The symptoms produced were similar to those in the last experiments; but the animal did not vomit, and the convulsive motions were in a less degree: he died eleven minutes after the poison was inserted. On opening the chest, the heart was found to have entirely ceased contracting; it was much distended with blood; and the blood in the cavities of the left side was of a scarlet colour. On irritating the heart with the point of the scalpel, the ventricles contracted, but not sufficiently to restore the circulation.

Experiment 24. *Exp.* 24. About a grain of the upas antiar was inserted into a wound in the side of a rabbit. He was affected with symptoms similar to those before described, and died in ten minutes after the poison was applied. On opening the thorax immediately after death, the heart was found to have ceased contracting, and the blood in the cavities of the left side was of a scarlet colour.

It appears to act like the infusion of tobacco, on the heart.

It appears from these experiments, that the upas antiar, when inserted into a wound, produces death (as infusion of tobacco does when injected into the intestine) by rendering the heart insensible to the stimulus of the blood, and stopping the circulation. The heart beats feebly and irregularly, before either the functions of the mind, or the respiration appear to be affected. Respiration is performed even after the circulation has ceased; and the left side of the heart is found after death to contain scarlet blood, which never can be the case, where the cause of death is the cessation of the functions of the brain or lungs. The convulsions, which occur when the circulation has nearly ceased, probably arise from the diminution of the supply of blood to the brain, resembling those, which take place in a person, who is dying from hæmorrhage.

How do poisons applied to wounds act on the brain?

There remains an interesting subject of inquiry, "through what medium do poisons influence the brain, when applied to wounds?" That poisons applied in this manner do *not* produce their effects precisely in the same way as *poisons* taken internally, is rendered probable by this circumstance; that

that some poisons, which are very powerful when applied to wounds even in small quantities, are either altogether inefficient when taken internally, or require to be given in very large quantities, in order to produce their effect; and *vice versâ*.

A poison applied to a wounded surface may be supposed to act on the brain in one of three ways, Three possible ways.

1. By means of the nerves, like poisons taken internally.

2. By passing into the circulation through the absorbent vessels.

3. By passing directly into the circulation through the divided veins.

Exp. 25. In order to ascertain, whether the woorara acts through the medium of the nerves, I exposed the axilla of a rabbit, and divided the spinal nerves supplying the upper extremity, just before they unite to form the axillary plexus. The operation was performed with the greatest care. I not only divided every nervous filament, however small, which I could detect, but every portion of cellular membrane in the axilla, so that the artery and vein were left entirely insulated. I then made two wounds in the fore arm, and inserted into them some of the woorara formed into a paste. Fourteen minutes after the poison was applied, the hind legs became paralytic, and in ten minutes more he died, with symptoms precisely similar to those, which took place in the former experiments, and the heart continued to act after apparent death. On dissection, the nerves of the upper extremity were particularly examined, but not the smallest filament could be found undivided. Experiment 25. The nerves divided, the effects the same.

I made the following experiment to ascertain whether the woorara passes into the circulation through the absorbent vessels.

Exp. 20. I tied a ligature round the thoracic duct of a dog, just before it perforates the angle of the left subclavian and jugular veins. I then made two wounds in the left hind leg, and introduced some of the woorara in powder into them. In less than a quarter of an hour he became affected with the usual symptoms, and died in a few minutes afterward. Experiment 26. The thoracic duct tied, the effects the same.

After

After death, I dissected the thoracic duct with great care. I found it to have been perfectly secured by the ligature. It was very much distended with chyle, and about two inches below its termination its coats had given way, and chyle was extravasated into the cellular membrane. The lymphatic vessels in the left axilla were distended in a very remarkable degree, and on dividing them, not less than a drachm of lymph issued from the divided ends.

The poison probably therefore passes through the veins.

Since neither the division of the nerves, nor the obstruction of the thoracic duct interfere in the slightest degree with the effects of the woorara, there is presumptive evidence, that it acts on the brain by entering the circulation through the divided veins. I endeavoured to ascertain, by experiment, whether this is really the case.

To apply ligatures to the large vessels of a limb only would evidently lead to no satisfactory conclusion, since the anastomosing vessels might still carry on the circulation. The only way, which I could devise, of performing the experiment, was to include all the vessels, small as well as large, in a ligature.

Experiment 27. The blood vessels included in a ligature,

Exp. 27. In order to make the experiment the more satisfactorily, I exposed the sciatic nerve of a rabbit in the upper and posterior part of the thigh, and passed under it a tape half an inch wide. I then made a wound in the leg, and having introduced into it some of the woorara mixed with water, I tied the tape moderately tight on the fore part of the thigh. Thus I interrupted the communication between the wound and the other parts of the body by means of the vessels, while that by means of the nerve still remained. After the ligature was tightened, I applied the woorara a second time, in another part of the leg. The rabbit was not at all affected, and at the end of an hour I removed the ligature. Being engaged in some other pursuit, I did not watch the animal so closely as I should otherwise have done; but twenty minutes after the ligature was removed, I found him lying on one side, motionless and insensible, evidently under the influence of the poison; but the symptoms were less violent than in most instances, and after lying in this state he recovered, and the limb became perfectly warm, and he regained the power of using it.

the animal not affected, till the ligature was removed.

Experiment

Exp. 28. I repeated the last experiment with this difference, that after having applied the poison, I made the ligature as tight as I could draw it. I removed the ligature at the end of an hour and twenty minutes, but the animal was not at all affected either before or after the removal of the ligature, and on the following day he had recovered the use of the limb. Experiment 28.

Exp. 29. I repeated the experiment a third time, drawing the ligature very tight. At the end of forty-five minutes, the animal continued perfectly well, and the ligature was removed. I watched him for three quarters of an hour afterward, but there were no symptoms of his being affected by the poison. On the following day the rabbit died, but this I attribute to the injury done to the limb and sciatic nerve by the ligature, as there was the appearance of inflammation in the parts in the neighbourhood of the ligature. Experiment 29.

These three experiments were made with the greatest care. From the mode, in which the poison was applied, from the quantity employed, and from my prior experience, I should have entertained not the smallest doubt of the poison taking effect in every instance in less than twenty minutes, if no ligature had been applied. In two of the three, the quantity of woorara was more than had been used in any former experiments. All confirm the opinion, that the poison enters the veins.

I have not judged it necessary to make any more experiments, with the ligature on the limb; because the numerous experiments of the Abbé Fontana on the ticunas coincide in their results with those, which have just been detailed, and fully establish the efficacy of the ligature, in preventing the action of the poison. It is not to be wondered at, that the ligature should sometimes fail in its effects; since these must evidently depend on the degree, in which the circulation is obstructed, and on the length of time during which the obstruction is continued. Abbé Fontana's experiments support the same conclusion,

There can be little doubt, that the woorara affects the brain, by passing into the circulation through the divided vessels. It is probable, that it does not produce its effects, until it enters the substance of the brain, along with the blood, in which it is dissolved; nor will the experiments of the Abbé Fontana, in which he found the ticunas produce almost even where they appear unfavourable to it.

almost instant death, when injected into the jugular vein of a rabbit, be found to militate against this conclusion; when we consider how short is the distance, which, in so small an animal, the blood has to pass from the jugular vein to the carotid artery, and the great rapidity of the circulation; since in a rabbit under the influence of terrour, during such an experiment, the heart cannot be supposed to act so seldom as three times in a second.

I have made no experiments to ascertain through what medium other poisons, when applied to wounds, affect the vital organs, but from analogy we may suppose, that they enter the circulation through the divided blood-vessels.

IV.

Death from destroying the functions of the brain.

The facts already related led me to conclude, that alcohol, the essential oil of almonds, the juice of aconite, the oil of tobacco, and the woorara, occasion death simply by destroying the functions of the brain. The following experiment appears fully to establish the truth of this conclusion.

Experiment 30.

Exp. 30. The temperature of the room being 58° of Fahrenheit's thermometer, I made two wounds in the side of a rabbit, and applied to them some of the woorara in the form of paste. In seven minutes after the application, the hind legs were paralysed, and in fifteen minutes respiration had ceased, and he was apparently dead. Two minutes afterward the heart was still beating, and a tube was introduced through an opening into the trachea, by means of which the lungs were inflated. The artificial respiration was made regularly about thirty-six times in a minute.

At first, the heart contracted one hundred times in a minute.

At the end of forty minutes, the pulse had risen to one hundred and twenty in a minute.

At the end of an hour, it had risen to one hundred and forty in a minute.

At the end of an hour and twenty-three minutes, the pulse had fallen to a hundred, and the artificial respiration was discontinued.

At the commencement of the experiment, the ball of a thermometer being placed in the rectum, the quicksilver

rose

rose to one hundred degrees; at the close of the experiment it had fallen to eighty-eight and a half.

During the continuance of the artificial respiration, the blood in the femoral artery was of a florid red, and that in the femoral vein of a dark colour, as usual.

Immediate cause of sudden death.

It has been observed by Mr. Bichat, that the immediate cause of death, when it takes place suddenly, must be the cessation of the functions of the heart, the brain, or the lungs. This observation may be extended to death under all circumstances. The stomach, the liver, the kidneys, and many other organs, are necessary to life, but their constant action is not necessary; and the cessation of their functions cannot therefore be the immediate cause of death. As in this case the action of the heart had never ceased; as the circulation of the blood was kept up by artificial respiration for more than an hour and twenty minutes after the poison had produced its full effects; and as during this time the usual changes in the colour of the blood took place in the lungs; it is evident, that the functions of the heart and lungs were unimpaired: but that those of the brain had ceased, is proved, by the animal having continued in a state of complete insensibility; and by this circumstance, that animal heat, to the generation of which I have formerly shown the influence of the brain to be necessary, was not generated.

Perhaps life might be preserved by keeping up artificial respiration.

Having learned, that the circulation might be kept up by artificial respiration for a considerable time after the woorara had produced its full effects, it occurred to me, that, in an animal under the influence of this or of any other poison, that acts in a similar manner, by continuing the artificial respiration for a sufficient length of time after natural respiration had ceased, the brain might recover from the impression, which the poison had produced, and the animal might be restored to life. In the last experiment, the animal gave no sign of returning sensibility: but it is to be observed, 1. That the quantity of the poison employed was very large. 2. That there was a great loss of animal heat, in consequence of the temperature of the room being much below the natural temperature of the animal,

animal, which could not therefore be considered under such favourable circumstances as to recovery, as if it had been kept in a higher temperature. 3. That the circulation was still vigorous when I left off inflating the lungs; and therefore it cannot be known what would have been the result, if the artificial respiration had been longer continued.

Experiment 30. Confirms this opinion.

Exp. 30. A wound was made in the side of a rabbit, and one drop of the essential oil of almonds was inserted into it, and immediately the animal was placed in a temperature of 90°. In two minutes he was under the influence of the poison. The usual symptoms took place, and in three minutes more respiration had ceased, and he lay apparently dead, but the heart was still felt beating through the ribs. A tube was then introduced into one of the nostrils, and the lungs were inflated about thirty-five times in a minute. Six minutes after the commencement of artificial respiration, he moved his head and legs, and made an effort to breathe. He then was seized with convulsions, and again lay motionless, but continued to make occasional efforts to breathe. Sixteen minutes after its commencement, the artificial respiration was discontinued. He now breathed spontaneously seventy times in a minute, and moved his head and extremities. After this, he occasionally rose, and attempted to walk. In the intervals, he continued in a dozing state; but from this he gradually recovered. In less than two hours he appeared perfectly well, and he continued well on the following day.

Inflating the lungs recommended in different instances.

The inflating the lungs has been frequently recommended in cases of suffocation, where the cause of death is the cessation of the functions of the lungs: as far as I know, it has not been before proposed in those cases, in which the cause of death is the cessation of the functions of the brain*. It is probable, that this method of treatment might

Experiments of Mr. Delile.

* Since this paper was read, I have been favoured by the Right Hon. the President with the perusal of a Dissertation on the Effects of the Upas Tieute, lately published at Paris by Mr. Delile; by which I find, that he had employed artificial respiration for the recovering animals, which were under the influence of this poison, with success. Mr. Delile describes the upas tieute as causing death by

might be employed with advantage for the recovery of persons labouring under the effects of opium, and many other poisons.

V.

The experiments, which have been detailed, lead to the following conclusions. General conclusions.

1. Alcohol, the essential oil of almonds, the juice of aconite, the empyreumatic oil of tobacco, and the woorara, act as poisons by simply destroying the functions of the brain; universal death taking place, because respiration is under the influence of the brain, and ceases when its functions are destroyed.

2. The infusion of tobacco, when injected into the intestine, and the upas antiar, when applied to a wound, have the power of rendering the heart insensible to the stimulus of the blood, thus stopping the circulation; in other words, they occasion syncope.

3. There is reason to believe, that the poisons, which in these experiments were applied internally, produce their effects through the medium of the nerves, without being absorbed into the circulation.

4. When the woorara is applied to a wound, it produces its effects on the brain, by entering the circulation through the divided blood-vessels; and, from analogy, we may conclude, that other poisons, when applied to wounds, operate in a similar manner.

5. When an animal is apparently dead from the influence of a poison, which acts by simply destroying the functions of the brain, it may, in some instances, at least, be made to recover, if respiration is artificially produced, and continued for a certain length of time.

From analogy we might draw some conclusions respecting the mode, in which some other vegetable poisons produce their effects on the animal system; but I forbear to enter into any speculative inquiries; as it is my wish, in the present communication, to record such facts only, as appear to be established by actual experiment.

by occasioning repeated and long continued contractions of the muscles of respiration, on which it acts through the medium of the spinal marrow, without destroying the functions of the brain.

III. *Description*

III.

Description of a Machine for Washing Potatoes and other esculent Roots for feeding Cattle: by Mr. William Lester, *of Paddington**.

SIR,

Machine for cleaning roots.

HEREWITH you will receive a machine for the more expeditious washing of all tuberous rooted vegetables (such as potatoes, turnips, carrots, &c.) from the soil that adheres to them when taken from the ground.

Disadvantages of an old one for the purpose.

These removed.

The staved cylinder, revolving in a trough of water so slow as not to excite the centrifugal force, is not new. I have made use of it myself twelve years ago, but always found it cold and wet work, to take the roots from it when washed. To obviate which, I have added the levers and wheels, and find it a very great improvement, as a boy therewith can do the work of two men, without exposing himself to the dangerous effects of dabbling in cold water. The importance of this mode will appear very obvious, when compared with the present laborious one used by the potato sellers in London. The partial motion given to the potatoes, by stirring them about in a tub, cannot separate the soil so effectually from them, as when the water is more violently agitated by their falling over each other in a revolving cylinder, neither will they be so much bruised as by the ends of the levers. If the soil should be particularly adhesive, the heads of a couple of old heath or birch brooms put into the cylinder will effectually disengage it from the eyes of the potatoes, and as the dirt separates, it falls to the bottom of the water in the vessel under the cylinder.

If you will have the goodness to lay this before the Society, and it should be deemed worthy of their attention, I will, if necessary, on being requested, attend to explain the effects of the machine.

I am, Sir,

Your most humble and obedient servant,

W. LESTER.

* Trans. of the Society of Arts, vol. xxvii, p. 34. The silver medal was voted to Mr. Lester.

SIR,

SIR,

AGREEABLE to your request I have procured the enclosed certificates, &c., on the utility of my improved root washer, which you will have the goodness to lay before the Society.

Great saving of labour in washing potatoes with it.

I have no doubt but it would save half the labour in washing potatoes in London, if it were brought into use: It is obvious to every one who has seen it work, that it is greatly superior to the tub and levers used by the potato-merchants, as it is not so liable to injure the roots. The soil is drawn from them with more facility, and their falling into the basket from the cylinder is more clean and commodious by far than taking them out of the tub with a grated shovel, from the corners of which many roots are bruised; it also prevents the potatoes being injured in quality from being long soaked in water, from which they suffer greatly in the common way.

I am, Sir,

Your most obedient and humble servant,

W. LESTER.

Certificates of the Utility of Mr. LESTER's *Machine for washing Tuberous Roots.*

SIR,

IN reply to your inquiries respecting the utility of the root-washer, which I purchased of you about twelve months since, I have much satisfaction in stating, that I have used it, constantly, during the last winter, and have found it to answer the purpose for which it is intended most thoroughly; and if my opinion will be of any benefit to you, I have not the least objection to your making it public.

Testimonies of its utility.

I am, Sir,

Your obedient humble servant,

JAMES JOHN FARQUHARSON.

SIR,

IN answer to yours I have to observe, I consider your root-washer to be a machine that no farmer, who is in the habit of giving roots to his stock, ought to be without. I use it constantly in washing potatoes for 150 fattening sheep, beside hogs. A man, and a boy ten years old, will wash,

 without

without any exertion, 20 bushels an hour, or a man alone will do half the quantity. I have tried a few parsnips with it, and find it do them equally as well, and have no doubt but any kind of roots may be washed with it. I am very much pleased with it, and so must every one who has tried it.

With every wish for your success,
I am, yours sincerely,
JOHN CLARKE.

To Mr. Lester, *&c.*

LORD NORTHAMPTON acquaints Mr. Lester, that the potato-washer, that was bought of him, answers the purpose perfectly well, and is approved by all who have used it.

Decription of Mr. Lester's *Machine for washing Potatoes, &c. Plate IX, Fig.* 1.

Description of the machine.

THIS machine is shown in plate IX, fig. 1. The potatoes are put into a cylinder or lantern A A, formed of two circular boards, and a number of staves connecting them. Six of these staves are connected at the ends of two pieces of wood, so that they can be opened as a door, to put in or take out the potatoes. The cylinder turns round in a trough B B, filled with water, and supported on four legs. On the end of the axis of the cylinder, two pulleys, one of which is shown separately at D, are loosely fitted; these are intended for the cylinder to move upon, when full of potatoes; they run upon a swinging frame E E, which rests on centres at F F: when the long end of the frame is pulled down, the other end is raised up, lifting the cylinder out of the trough B B; when the long end of the frame becomes the lowest, the cylinder rolls down on its wheels D, till it is over the hopper or wooden funnel G, under which a wheelbarrow or basket to receive the clean potatoes is placed: the door of the cylinder is now opened, and the contents turned out through the hopper into the vessel beneath it. When the frame is in this situation, the iron rods H, which are jointed to the short ends of the levers, form stops to the farther descent of the frame.

Mode of using it.

When fresh quantities of potatoes are to be washed, they are thrown in at the door of the cylinder, which is then shut

shut up, and held shut by two small bolts. The end of the frame E is then raised up, so as to make the short end the lowest, and the cylinder runs down on its two wheels D over the trough B, till it is stopped by two iron prongs fixed on the end of the frame E; the cylinder is then suffered to fall down into the trough, and the potatoes, &c. are washed by turning it round by its handle K. *a* is a plug to let out the foul water.

Any person who has seen the laborious and imperfect method of washing potatoes in a tub, as practised in London, will be convinced of the utility of this machine, and of its preserving the potatoes from being water soaked and spoiled, which is the case when they are long immersed in water. Advantages.

IV.

Method of packing Plants and Trees intended for Exportation, so as to preserve their vegetative Powers for many Months: by WILLIAM SALISBURY, *of the Botanic Gardens at Brompton and Sloane-Street* *.

SIR,

WHEN I had the pleasure of seeing you last Spring, I mentioned a supposed discovery I had made of a substance, that would preserve trees and plants for a considerable time in a growing state, when packed up in close boxes: and that by this method they might be sent abroad to great distances with more success and less trouble than in any other. I now take the liberty of troubling you with the results of several experiments, which I have since made; being certain, that a greater demand will be found for the various articles cultivated in this country, and the persons who are engaged in that trade benefitted, when it is publicly known. Discovery of a substance that will preserve plants in a growing state several months.

A box I have now sent, marked No. 1, contains specimens of tulip trees, and liquid amber trees, which were Trees packed in it six months.

* Trans. of the Soc. of Arts, Vol. XXVII, p. 40. Twenty guineas were voted to Mr. Salisbury for this communication.

d up close from September, 1807, till March, 1808: were then planted in my nursery; and the whole, ating to several hundreds, have grown equally as well y would have done, if only transplanted from one part other of the same ground.

Several trees sent to America.

February last I sent to Boston in New England two pac [illegible]ges in this way, each containing upwards of nine hundred [illegible]rees of different kinds; and I have lately received the pl[illegible]ng intelligence, that they have all arrived safe and done well, but that some fruit trees sent to the same gentleman packed in the usual way, were all spoiled, owing to [illegible] sel, in which all the packages w[illegible]

Trees packed four months or more.

[illegible] box I [illegible] to you, marked No. 2, con[illegible] rees, which were packed up [illegible] have been in the boxes four [illegible] eriod, and the remainder now [illegible] xes are [illegible] state of preservation, and I [illegible] doubt [illegible] three months longer, or more, [illegible]ury.

Cause of trees being injured in long journeys.

I must beg leave to observe, that the principal cause why things of this nature do not succeed in long journeys is, that if the package, (as is commonly the case) becomes by any means damp, it is very liable to heat, and the contents to be thereby very much injured: and if left dry, the moisture of the trees becomes exhausted, and they consequently die for want of nourishment. The mode recommended some years ago by Mr. Ellis, of planting the articles in tubs or boxes of earth, is attended with so much trouble, that it has been seldom found to succeed.

Planting in boxes of earth too troublesome.

Properties of the sphagnum palustre, or common bog moss.

In packing my plants, I make use of the long white moss, the *sphagnum palustre* of Linnæus, which grows in great plenty on peat bogs, and, when decayed, forms a great portion of that substance. It differs materially from other vegetables in possessing the power of retaining moisture in a wonderful degree, and it does not appear to be liable to fermentation in any situation, even when laid together in great quantities; hence a decomposition does not readily take place, and it preserves the power of affording moisture

and

and nutriment to plants when completely enveloped in it, as appears by the above experiment.

I am, with great respect, Sir,

Your very obedient, and most humble servant,

WILLIAM SALISBURY.

WE hereby certify that we packed up the several trees and plants at the times marked in the labels of those in the box No. 2, by desire of Mr. Salisbury, and that the said specimens have remained ever since in the boxes as above described.

ALEXANDER REITH.

JOHN WOODHOOD.

DEAR SIR,

THE prosperity of a country was never more rapidly promoted, than we have happily witnessed in our own nation within a few years, since the study of natural history has become so general among all ranks of society; and probably nothing has contributed so much thereto as the extended knowledge of botany, and the numerous collections of vegetable productions, which have lately been introduced from all parts of the world. From such sources our agriculture, and many of the arts, have been greatly improved; yet much still remains to be accomplished by the assiduous botanist; for instance, neither the plants producing the cinchona, or which nourish the cochineal, have yet reached our soil, nor are we even acquainted with those which yield many of our most useful drugs. This is owing, in a great measure, to the difficulty of procuring perfect seeds, it being a well known fact, that many kinds will not vegetate, if left dry but a short time after gathering; and the difficulty of keeping plants alive during long voyages has been almost an insuperable obstacle. Impressed, therefore, with the importance of the subject, I wrote to you on the 9th of January last, and have now the pleasure of communicating to the Society of Arts, &c., for the benefit of the public, farther particulars of the mode I have discovered; and by which I am convinced, from actual experiments, trees or plants of all kinds may, with ease and certainty, be transported from any part of the globe to this country and our colonies; being confident, that our commerce will be improved

Benefits of the study of natural history.

Much still wanting

from the difficulty of keeping seeds

and plants alive.

improved by a more certain mode of exporting the numerous fruits with which our nurseries exclusively abound.

The sphagnum palustre preserves plants in a vegetating state.

I had, some time ago, an opportunity of viewing a large heap of moss (*sphagnum palustre, Linn.*) which had been collected for decorating a grotto. I observed, that, although it had lain exposed for several months in the heat of summer, yet, with the exception of the very outside of the heap, its particles appeared in the same state as when first collected, and that a gentle state of vegetation was still going on. I moreover observed, that several species of heaths, grasses, and plants, that had been by chance collected in the heap, were preserved, and in several instances had the same appearances as when growing; others were a little blanched for want of light; but even these were alive, and capable of growing by proper management. These circumstances led me to make some experiments to ascertain how long trees of different kinds might be preserved in this substance, when excluded from the external air; and I so far succeeded, as to keep them for six months, part of which time had been extreme hot weather, and I had afterward the pleasure of getting them to grow in my garden equal to any, that had been transplanted the same season.

Experiments on it.

Other mosses do not answer the same purpose.

As I have endeavoured to discover what property this particular moss possesses, when compared with others generally used for packing plants, I shall remark, that, as its name implies, it is in a great measure an aquatic, and consequently not liable to injury from moisture, which it has the power of retaining in a wonderful degree, while all the species of hypnum cannot be prevented from rotting, unless they are kept perfectly dry; and although the mosses in general, when moistened with water, are useful to wrap round the roots of trees when packed up, yet they gradually undergo a decomposition; and consequently, if plants were completely enveloped therein, they would decay in time from the same cause, which I have proved in many instances.

I was therefore led to ascribe the advantages, which the *sphagnum palustre* possesses, to its property of holding water, and resisting fermentation; and I am confirmed in this opinion, by a letter, which I have received from my worthy friend

friend Mr. A. T. Thompson, to whom I had submitted some of that moss, for a chemical analysis, and whose letter I now enclose.

Mode of packing plants in it.

The manner in which I have been accustomed to pack up plants is as follows. When the moss is collected from the bogs in which it grows, it should be pressed, in order to drain out as much moisture as possible, and having boxes prepared of sufficient sizes for the young trees, (which may in some instances be shortened in their branches), I lay in the bottom of the box as much moss as will, when pressed with the foot, remain of the thickness of four inches. A layer of the plants should then be put thereon, observing that the shoots of each other do not touch, and that the space of four inches be left round the sides; after this, another layer of moss, about two inches thick, is placed, and then more plants; and I thus proceed, till after the whole of the plants are pressed down as tight as possible, and the box filled within four inches of the top, which space must be filled with the moss: the contents are then trodden down with the foot, and the box nailed closely up.

Treatment of the trees when they arrive.

When trees are intended to be sent to distant countries, I should advise such to be selected as are small and healthy; and when arrived at their place of destination, they should be cut down quite close, even to the second or third eye from the graft, or, in trees not grafted, as near the former year's wood as possible; and having prepared beds according to the following mode, let them be planted therein, to serve as a nursery; for trees of every description suffer so much from removal, that, unless the weather is particularly favourable, they do not recover it for some time, even when only transplanted in their native climate. I do not think it advisable, therefore, to plant them at once, where they are liable to suffer from want of water, and other attentions necessary to their perfect growth. I therefore recommend beds to be thus prepared for them, viz. On some level spot of ground, mark out beds five feet wide, and leave walks or alleys between them, of two feet wide, throwing a portion of the earth out of the beds upon the alleys, so as to leave them four inches higher than the beds.

If

If the ground is shallow, and the under stratum not fit for the growth of trees, the whole should be removed, and the beds made good with a better soil.

Advantages of this mode.

The advantage arising from planting trees in this way is, that, the beds being lower than the walks, the water which is poured on, for support of the trees, is prevented from running off. The plants are also less exposed to the influence of the winds; and, if a dry and hot season should immediately follow after they are planted, hoops covered with mats, straw, or canvas, may be placed over them, to prevent the sun from burning the plants, and to hinder a too speedy evaporation of moisture.

Shades for the trees.

In warm climates, canvas cloth will answer best for these shades, to be fixed during the heat of the day, so as to prevent the surface of the mould from becoming dry; and if a little water be sprinkled upon the canvas, once or twice during the day, it will keep it tight, and produce a moist atmosphere underneath, which will greatly facilitate the growth of the plants.

These shades should be removed at the setting of the sun, and the plants then watered, when they will also receive the benefit of the dews during the night. In the morning the shades should be replaced, and the plants thus protected till they can stand the open air, to which they should gradually be inured by removing the shades daily more and more, till they can be wholly taken away.

Distance of first planting.

The plants should be planted in rows across the beds, about three inches distance from each other, and the rows should be about nine inches apart; and when the plants have grown thus for one year, they may be removed to the places where they are intended to remain.

I remain, dear Sir, your obedient servant,

WM. SALISBURY.

DEAR SIR,

Analysis of the sphagnum palustre.

THE analysis of the moss, which you put into my hands, has afforded the following result.

A portion of it macerated in boiling distilled water, for twenty-eight hours, yielded a pale straw-coloured, slightly mucilaginous infusion, which was nearly insipid, and of a disagreeable odour.

The

The infusion of litmus was reddened when added to it. With the nitrate and acetite of barites, insoluble precipitates were thrown down, as was also the case with the acetite of lead. Sulphate of iron gave a very slight olive tinge to the infusion, after standing eight hours; and with the solution of gelatine, a small quantity of a whitish flocculent precipitate was formed, after standing twelve hours. The oxalic acid, a solution of pure ammonia, and the nitrate of silver, produced no effect on the infusion.

The conclusion to be drawn from these results is, that the moss contains in its composition, beside the ordinary principles of vegetables, a very small portion of gallic acid, and of tannin, some sulphuric acid in an uncombined state, mucilage, and extractive matter. No inference can, therefore, be drawn from these results, which explains in any degree the effects of the moss in preserving the vegetables that are enveloped in it; nor is there any effect produced in the air by it, more than is produced by mosses in general, when in an uncorrupted state; other causes to explain the preservative property of the moss must therefore be looked for, and these are to be found, in my opinion, in the peculiar qualities of the moss, connected with its own existence as a living plant.

Plants will not vegetate unless kept alive.

Plants which are taken from the earth, and packed up to be sent abroad, or to any distance so considerable as to keep them for some length of time in the package, will not vegetate when again taken out of it and planted, unless some degree of vitality has been preserved during the period that they have been out of the ground.

Circumstances requisite to this.

To preserve this, four circumstances are essential in the packing material; softness, in order that the delicate parts of the enveloped vegetable be not injured; looseness, that a certain portion of air be contained in it, and that an equal temperature may be preserved; moisture; and the power of resisting fermentation, and the putrefactive process.

Bog moss eminently possesses these.

All of these circumstances this moss possesses in a remarkable degree; its power of absorbing and retaining moisture is more considerable than that which perhaps any other moss possesses, it is light, soft, and loose in its texture, and its vitality is so considerable, as to carry on the powers

of

...itation, and consequently to enable it to resist fer... ...on and putrefaction for a very great length of time. ...d under such circumstances, the plants, which are ... up in the moss, enjoy a kind of life in some degree ... to that enjoyed by an animal in a torpid state, the ...ns of life are supported at a very low state, but still ...t to preserve them in a situation to be acted upon ...urable circumstances, when again planted. Such is ... ...ory I have formed of the effect of this moss in pre... plants; the many necessary calls of my profession ...t allowed me time sufficient to investigate the sub... ... could have wished to have ... plead my apology, for the ... ...nion is presented to you. I ... ...a value, both to botany and ...

Theory of its action.

...eve me,

Yours truly,

A. T. THOMPSON.

Dear Sir,

Various fruit trees sent to Sierra Leone, packed in this moss,

IN addition to the account which I delivered *to you*, respecting my method of packing plants for exportation in the sphagnum palustre moss, I beg leave to observe, that, at the time the case was packed up, which I sent to the Adelphi in January last, a similar package was sent from me to Sierra Leone, by desire of the African Institution, who wished to introduce into that colony the mulberry tree for feeding silk worms; also different kinds of vines, and other fruit trees, amounting in the whole to nearly fifteen hundred trees.

with success.

They arrived there in about four months after the package was made up, and the trees were planted under the direction of a gentleman, to whom I gave a copy of the instructions, which accompanied my former letter to you of last January. The following account of them has since appeared in the African Herald. "A number of fruit and other trees, among which are the white and red mulberry, vines, &c., have been sent from London, by order of the African Institution; all of which are at present growing here,

here, in a very flourishing state; and a piece of ground is clearing in the mountains, to which they are intended to be removed the next season."

I requested the gentleman, to whose management the plants were entrusted, to acquaint me how they succeeded, and to use the same moss in packing up for me some of the wild plants of that neighbourhood, which he did in June last; and at the same time I received a letter from Mr. Macaulay of that place, with the following intelligence. "The plants which were bought of you, and sent out by the African Institution, all thrive very well, except the tea tree, sour sop, and a few others. The mulberries, &c., grow most luxuriantly; most of the trees have been removed to a more temperate situation, about three miles hence, where the remainder will soon also be planted." African plants sent to England in the same moss,

This letter arrived by the Derwent, captain Colombine, who also brought me a box of plants packed up in the moss, which had been previously sent with the above; and although the package did not arrive at Brompton before the 5th of October last, the plants were in a fine state of vegetation, and are now growing in my hot-house; and even the moss itself had preserved its vegetative state, and was perfect. and arrived in a fit state for vegetating.

I have been thus particular in my description of the fact, as it is a corroborating proof of the utility of this moss for such purposes; and as the removal of trees cannot be otherwise effected in long voyages, without great expense and inconvenience.

I am, with great respect, Dear Sir,

Yours very truly,

WILLIAM SALISBURY.

Reference to Mr. Salisbury's Method of managing Plants, after they are removed from the Package. See Plate IX, Fig. 2, 3.

The plan fig. 2, at the bottom of plate IX, represents, on a small scale, sections of the beds and alleys, with the plants as first set. The beds, *a a*, are to be made on level ground, each bed to be five feet wide, with a space, *b b b*, between Method of managing the plants after their arrival.

n each for a road. A portion of the earth is to be out of the five feet beds, upon these roads, so as to em four inches higher than the beds, as shown in the C represents the plants as first set out, with an arched f canvas cloth over them; D shows the plants when ve grown in the beds for some time, and in a state rea or planting out.

Directions for pruning them.

1 llustrate the mode of cutting or pruning the plants, afte ey are removed from the package: fig. 3, No. 1, is ed to be a fruit tree, of one year's growth from the that is a maiden plant. It is to be cut down as is re- pr xt season's growth will form the to remain as a dwarf, or if s is repr . 4, it will form a standard, ny in orchards with high stems, out of the re of cattle. No 5 is supposed ut any small at has not been grafted, but a for planting. 6 is the form it will make the g season, whe nay be left; or, should it be in- tended for timber, o re a crooked stem, cut it close down to the ground as at No. 7, and it will throw up several shoots, which should be all cut off but the *strongest*, and that will make the tree No. 8. This may afterward be kept trimmed up to a single stem, and a tree be formed much better than in any other mode.

Plants kept alive in the moss nine months.

N. B. The packages of plants, Nos. 1 and 2, mentioned in Mr. Salisbury's first letter, were opened and examined by the committee of agriculture, on the 16th of January, 1809, when all the plants appeared to be in a state fit for vegetation. The boxes were then closed, and placed in the society's model room, and opened again on the 30th of May, at the distribution of the rewards of the society; the plants were then in a state fit for growth, having formed both new roots and branches during their confinement. It appears, therefore, that the plants were, from their first enclosure by Mr. Salisbury, thus preserved nine months out of the earth.

V. *Description*

V.

Description of an Apparatus used at Sheffield for cleaning Chimneys: by Mr. Samuel Roberts, *Chairman of a Committee appointed at that Place for encouraging the Sweeping of Chimneys without the use of Climbing-boys*.*

THE two brushes, Plate X, fig. 1, and 2, are those which at present appear to answer best the intended purpose. Fig. 1 is the easiest to work in difficult chimneys; but in those which are tolerably straight No. 2 will be found the more convenient, as it clears itself better of the soot in ascending. Soldered on the inside of the iron hoop A, at *b* is a hollow iron tube, going through the wooden balls B. The nut C screws upon the upper end of the hollow tube, through which the rope passes, and fastens the whole together. The balls B are put upon the tube in separate parts, divided at *d*, for the conveniency of putting in and replacing the brush part F, which is composed of bristles, whisk, and whalebone. The whisk (which should be well selected for the purpose) is in the middle, on each side of which, above and below, is a row of whalebone, split thin, with the flat sides towards the whisk, and above and below the whalebone are bristles. Care must be taken that the whole is not too thick and strong, otherwise it will be difficult to get in and out of the pipes on the tops of the chimneys; where they are pressed together between the balls B, they should not be thicker than three eighths of an inch. Great care must also be taken, that the parts of the brushes are well fastened together, and firmly fixed between the balls B, so as not to be loosened in working. The diameter of the balls B is three inches. The distance

Apparatus for cleaning chimneys.

* Abridged from the Trans. of the Soc. of Arts, vol. xxvii, p. 209. The Society, anxious to relieve the sufferings of humanity, have attended with much pleasure to the endeavours of the inhabitants of Sheffield, and cooperate with them in their attempts to supersede the necessity of employing climbing boys; they have, therefore, immediately on receiving the following communication, ordered it to be inserted in their volume, and an explanatory engraving of the machinery employed to be annexed.

between

en the two brushes FF, in drawing fig. 2, is about inches. The wooden tubes D, (which are about one n diameter,) through which the rope passes, should e too long; the shortest next the brush should not l fifteen inches. They should gradually increase in as they recede from the brush to the bottom, where hould not exceed thirty inches. The brush, fig. 3, od deal similar to a bottle brush, the handle about eet long, made of whalebone, wrapped with iron or wire, the brush part made of bristles only. It will nd to be very useful in cleaning short flues, &c. in

Contrivance f preventing soot from f into the ro...

thin which the machine may useful in rooms, where it is ent the least particle of soot ... The ... rs E are of oak, about one half an inch thick, turning ...n-pin re two small iron rods, slip- upon pegs a... of which is suspended a linen ..., the one ...imney, H, a short one, the other, as shown by the ... line I, a long one, reaching to and resting five or six inches upon the floor. *ee* are small pegs, which, when the bars E are closed, fit into the notches *gg*, so as to stop the bars in the proper place, and prevent their being opened the wrong way. When the bars E are opened, they stretch the tapes K, which are fastened to the tops of the bars *h*, and are about three feet six inches long, to which extent only they suffer the bars to open. When thus extended, and placed in the proper situation, a loose sheet, of the same kind of linen as the curtains, is thrown over, and hangs down over the tapes, and upon the floor at each end, buttoning to the curtains at the corners, so as to form a complete tent, about five feet long, four feet wide, and five feet high, within which both the man and the boy can stand with the machine to work it.

VI. *Abstract*

VI.

Abstract of a Paper on the bitter Substances formed by the Action of Nitric Acid on Indigo: by Mr. CHEVREUL *.

§ I. 1. BEFORE I recite my experiments on the bitter and acid substances, that are obtained by treating indigo with nitric acid, I will briefly advert to the labours of others on the same subject at different periods.

Action of nitric acid on indigo examined.

2. Mr. Haussman was the first, who made known the formation of the bitter principle by the action of nitric acid on indigo. Mr. Welther afterward obtained it from silk by means of the same acid, described its principal properties, and gave it the name of *amer*.

Amer from it, and from silk.

3. Messrs. Proust, Fourcroy, and Vauquelin, have shown in several papers, that almost all organic substances, into the composition of which nitrogen enters, yield Welther's amer, and frequently benzoic acid.

Almost all organic substances yield it, and frequently benzoic acid.

Messrs. Fourcroy and Vauquelin studied with great attention the properties of the amer obtained with indigo. They observed, that it was acid; and that it was to be considered as a superoxigenated hydrocarburet of nitrogen, forming with pure potash a detonating compound, which appeared not to contain any nitric acid, as Welther had said. They observed farther, that, if the action of the nitric acid on the indigo was stopped, before the whole of the amer was formed, an acid was obtained, which sublimed in white acicular crystals, and appeared much to resemble benzoic acid.

Properties of amer according to Fourcroy and Vauquelin.

4. Mr. Hatchett, in his learned researches concerning the action of sulphuric and nitric acids on vegetable compounds, made known several products, that precipitate gelatine as tannin does: and on account of this property, combined with several others, he called them artificial tanning matter.

Mr. Hatchett's artificial tannin.

5. I observed in the year 1808, that the extract of Brazil wood was converted by nitric acid into a bitter substance,

Action of nitric acid on brazil,

* Ann. de Chim. vol. lxxii, p. 113. Read to the National Institute, the 30th of Nov., 1809.

that

that differed from the amer of Welther: and considered it as a compound of *nitric acid, amer, and artificial tannin.*

and on aloes.

6. Mr. Braconnot, in a paper on gum-resins, speaks of an *acid,* which he obtained with aloes and nitric acid. He remarked, that this acid bore some analogy to the amer of indigo, and also to an orange-coloured substance, that Messrs. Fourcroy and Vauquelin had formed with muscular flesh and nitric acid*.

Supposed acid from indigo.

7. In the month of January, 1809, I resumed my examination of the amer of Brazil wood, in order to find how far it resembled the aloetic acid of Mr. Braconnot, when Mr. Vauquelin communicated to me a letter, in which he was informed, that Mr. Moretti, professor of chemistry at Udina, had obtained, by distilling indigo with nitric acid, an acid, that formed a detonating compound with potash, soda, the oxides of iron, lead, silver, &c. It was added, that Mr. Moretti considered it as a new acid, because it could not be confounded with the benzoic, which Messrs. Fourcroy and Vauquelin said they had formed with indigo. Mr. Vauquelin was desirous, that I should repeat these experiments; and at the same time requested me to examine, whether these acid and detonating products *did not owe* their properties to some nitric acid, which they retained in combination.

Inquiry instituted concerning it.

Indigo treated with nitric acid.

§ II. 8. Into a retort I poured four parts of nitric acid at 32° [1·283] diluted with four parts of water. A receiver being fitted to it, I placed it on a moderately warm sand heat, and added gradually two parts of Guatimala indigo coarsely pounded. The mixture grew hot, and a quantity of nitrous vapours, carbonic acid, &c., was evolved. Fearing the action would become too violent, I removed the apparatus to a cold sand bath, and left the substances to themselves for four and twenty hours.

Products distilled over.

9. During this time nitric acid, prussic acid, and a small quantity of yellow bitter matter, had passed into the receiver.

Matter in the retort.

10. The liquor, that remained in the retort, was of a reddish yellow, and two concrete substances floated on it.

* See Journal, vol. xxvii, p. 361.

The most abundant had the appearance of a *resin.* The other was of an orange-colour, and disseminated in this in the form of clots. These were both separated from the liquid, washed with cold water, and then boiled. The resinous matter congealed on cooling; and the orange-coloured substance, after having dissolved, fell to the bottom in small grains, which did not adhere to each other.

11. The water, that had been employed to separate these two substances, was added to the liquor (10) left in the retort, and then distilled. Nitric acid, prussic acid, amer, and a little ammonia, passed into the receiver. The concentrated liquor on cooling let fall crystals formed of the *amer* of Welther, and of the *benzoic acid* of Messrs. Fourcroy and Vauquelin. Having dissolved these in hot water, I obtained by cooling the *crystallized acid*, retaining a little amer; and by evaporating the liquid fine yellow scales of amer.

The liquid distilled. Products. Yielded crystals of amer and an acid.

12. The liquid, which had furnished the crystals (11) of amer and acid, after boiling down let fall a *red liquid substance resembling a fat oil.*

Fat oil from the mother-water.

13. The supernatant liquor (12) was evaporated to dryness, and hot water poured on the residuum. *Oxalate of lime* was left undissolved; and the water, on cooling, let fall some oily matter, and afterward a *yellow sediment, which was pretty soft*, and differed from the oily matter only in the proportion of its principles.

More of this, and oxalate of lime.

14. I shall now proceed to examine, 1st the amer; 2d, the acid substance, which has been compared to benzoic acid; 3d, the resin. The other products being only compounds of these three, I shall not speak of them under separate heads; but I intend in a future paper to return to the *substance of an oily appearance.*

The products examined.

§ III. Art 1. *Of the Amer.*

15. The scales of amer, which I mentioned (11), retained a little resin, whence they derived a deep yellow colour; and a small quantity of the acid, which has been called the benzoic, but which I shall designate under the name of *volatile acid.*

The amer impure.

Its properties when pure.

When the amer is very pure, it is white inclining to straw-colour. Its solution in water is not reddened by salts of iron at a maximum. That which was employed in the following experiments had been boiled in nitric acid: afterward crystalized repeatedly; combined with potash, and then separated from it by muriatic acid; and lastly crystallized, till, when redissolved in water, it no longer precipitated solution of silver.

Action of heat on it.

16. The amer, being gently heated in a common phial, sublimed in little needles, or scales, of a white colour inclining to straw-yellow. Thrown on a redhot iron it took fire, and left a coal, which melted. If exposed to a red heat in a retort, a pretty strong smell of nitrous acid and of prussic acid is evolved.

Apparatus for examining its products.

To examine the products of the amer subjected to the action of heat, I contrived an apparatus consisting of a glass ball surmounted with a tube, which terminated under a jar filled with mercury. Into the ball I introduced 2 dec. [3 grs.] of amer, more would have burst it, and fastened the tube to the jar by means of a wire.

Heat applied.

17. When the apparatus was thus arranged, I heated the ball with a redhot coal: the amer melted, grew black, and took fire; a light coal remained; and aqueous vapour, gas, and a little charcoal passed into the receiver.

Gaseous products.

The gaseous product reddened litmus paper. It had the smell of nitrous acid, mixed with that of prussic; and I analysed it in the following mode. I first passed some water into the jar, and a slight absorption took place.

Carbonic and prussic acids.

When this appeared to be at an end, I shifted the water to another jar filled with mercury; and found, that it had dissolved a portion of amer, which had been volatilized without decomposition, some carbonic acid, and some prussic acid.

Mode of detecting the latter.

To detect the latter it was necessary, to saturate the liquid with carbonate of potash; and pour it into a small glass retort adapted to a receiver, in which were some threads twisted together, impregnated with green sulphate of iron, and afterward dipped in a weak solution of potash. On distilling, water and prussic acid passed over; and the thread, after having been washed with weak muriatic acid, became blue. (If sulphate of iron

were

were mixed directly with the liquid saturated with potash, no prussian blue was obtained).

The residuum of the distillation was reddish, and gelatinized. Sulphuric acid evolved from it a smell of prussic acid, mixed with that of nitrous acid. Residuum.

The water therefore had dissolved, beside the undecomposed amer, *carbonic acid*, *prussic acid*, and *nitric acid:* and I have every reason to think, that it contained a little *ammonia*. Nitric acid and a little ammonia.

18. The gaseous residuum insoluble in water was placed in contact with a solution of potash for twenty-four hours, in order to abstract from it the carbonic and prussic acid, that it might still retain. After this it was carefully washed. In this state it did not redden infusion of litmus; but as soon as it was exposed to the air, it reddened it, and produced a pretty strong smell of nitrous acid; so that it must have contained *nitrous gas*. It burned in the same manner as *oily hidrogen*. I had observed several times, that this residuum extinguished burning substances, because then it contained a great deal of nitrogen gas; and at other times, that it burned like carbonic oxide gas. It appeared, that these differences were owing to the degree of rapidity, with which the amer was decomposed. The gaseous products not soluble in water were

The gaseous residuum insoluble by water or potash, therefore, consisted of *nitrous gas*, *inflammable gas*, and *nitrogen*. As I was not able to make an accurate analysis of this residuum, I cannot say whether the whole of the nitrogen came from the air in the apparatus, the oxigen of which was converted into nitric acid by the nitrous gas; or whether part of it arose from the decomposition of the amer. nitric oxide, hidrogen, and nitrogen.

19. From these facts it appears to me, that *amer* is a compound of *nitric acid*, and a *vegetable matter*, probably of an oily or resinous nature. Perhaps it may be objected, that the nitrous gas obtained may be formed during the process, by the compression the gasses undergo: as is the case when hidrogen is detonated with oxigen containing nitrogen in Volta's eudiometer, or in a glass globe for the recomposition of water: but the compression of the gasses in these apparatuses appears to me to be much greater, than Composition of amer.

they

they experience in the detonation of amer. However, the facts that follow, and those which I purpose to relate, will show, that it is more natural to consider amer as a compound of nitric acid, than as a substance formed directly of oxigen, hidrogen, carbon, and nitrogen.

Action of potash on amer. 20. Amer is much more soluble in hot water than in cold. Its solution is acid, very bitter, and even a little astringent. If it be mixed with a concentrated solution of potash, small needly crystals of a gold colour are obtained, which are a compound of amer and potash, and have been described by Welther, Fourcroy, and Vauquelin. These crystals detonate loudly when heated. They cannot be heated in a glass ball without breaking it to pieces. If 15 cent. [2grs.] be heated in a small assay matrass, a loud detonation is produced, the vessel is filled with soot, and a smell of prussic acid is emitted. If the matrass be closed as soon as the detonation has taken place; and, when it is cool, a solution of potash be poured in, and afterward of green sulphate of iron, prussian blue will be obtained.

A detonating compound formed.

Dissolved in water. 100 dec. of boiling water dissolved 7 dec. of the detonating matter. On cooling, a great part of the latter separated in the form of small needles. The solution was not acid, and did not appear alkaline.

Properties of this compound. This compound is decomposed by nitric or muriatic acid at a boiling heat, as Messrs. Fourcroy and Vauquelin observed; and, on cooling, the amer crystallizes in white scales, inclining to straw-colour. But a very remarkable fact, which proves, that there is no such thing as elective attraction, is: if you take a solution of potash, supersaturated with nitric or muriatic acid, mix with it amer, evaporate to dryness in a small capsule, and dissolve the residuum in hot water, you will obtain on cooling small detonating crystals, of a gold colour, formed of amer and potash; whence it follows, that amer decomposes nitrate and muriate of potash.

No such thing as elective attraction.

Causes of these opposite effects. Thus we have two opposite effects, that occur within a range of temperature the extremes of which are by no means remote; and which are easily explicable, if we attend to the circumstances. In the first, we perceive that the amer must first separate, since potash is capable of forming with the

the nitric or muriatic acid a compound more soluble in water than amer: the latter therefore is separated by the force of crystallization. In the second experiment, the amer and potash, being more fixed than the nitric or muriatic acid, combine; while the acid flies off by the expansive power of heat.

Amer combines very well with ammonia, and the result is small yellow scales, which scarcely detonate on being heated. Union of amer with ammonia,

21. Amer unites with lime, barytes, and strontian, and forms compounds soluble in water. It requires but a very small quantity of lime, or even of carbonate of lime, to turn the crystals of pure amer yellow. The mere contact of common paper is sufficient, to produce this effect. the earths,

22. Amer dissolves oxide of silver, and forms with it needles of a rich gold colour; but they grow black by exposure to air. A similar compound may be obtained by pouring amer into a solution of silver, and leaving it to evaporate spontaneously. oxide of silver,

It dissolves carbonate of lead, and forms with it acicular crystals, which are not very soluble, unless they contain an excess of amer. carbonate of lead,

It equally dissolves red oxide of mercury. and oxide of mercury.

All these compounds detonate when heated.

23. The theory of the detonation of amer is easily comprehended. When its temperature is raised, part of the nitric acid is converted into nitrous gas. The other and greater part is wholly decomposed; the oxigen of the acid attacks the combustible principles of the vegetable matter, and forms water with the hidrogen, and carbonic acid with the carbon; the whole, or part, of the nitrogen of the nitric acid forms prussic acid, and perhaps ammonia, with some of the hidrogen and carbon; another portion of hidrogen, uniting with carbon, forms oily inflammable gas. The residual coal is so much the more bulky, in proportion as the amer has been less strongly heated, because then the most dilatable principles are first evolved. Theory of its detonation.

One thing to be observed in the action of a gentle heat on amer is the fixedness, that the constituent principles of nitric acid appear to have acquired in this compound: for Fixity of the nitric acid in amer.

we find, that combustion does not take place, till the charcoal is predominant.

This confirms the opinion of forming a chemical union with it.

This fact appears to confirm the existence of nitric acid in amer; because it seems, that the oxigen, if it were combined directly with the carbon, hidrogen, and nitrogen, would attack the hidrogen, as soon as the combination between the principles of the amer was loosened by heat, rather than remain attached to the carbon, waiting till the temperature of the latter was sufficiently raised, to admit its combining with it*. On the hypothesis, that amer is a compound of nitric acid with a combustible formed of hidrogen and carbon †, we can better understand what passes, on heating it gently. In this case, part of the hidrogen, a little carburetted, is evolved at a temperature too low to separate the principles of the nitric acid, so that the nitric acid, when it has reached the temperature at which this separation is effected, acts on a combustible, that has already lost a part of its hidrogen, and consequently finds itself more carburetted than it was before.

The detonating power of amer

The detonation of amer ought to be loud, because it contains oxigen enough to saturate the greater part of its combustible elements, and form gaseous compounds with them: but, as it is volatile, it follows, that one portion escapes combustion: and that this takes place successively, because the heat is not uniform. Thus we can account for the effect produced by an alkaline base, when it is combined with amer, and when this compound is made to detonate. In this case, the amer, being rendered more fixed, becomes much more detonating; because, as the heat is allowed to accumulate, its elements separate simultaneously, and thus produce a more forcible detonation. This is the way in which Messrs. Fourcroy and Vauquelin view the action of the base; and in proof of its truth it is to be observed, that the detonation is in general so much the stronger, in proportion as the base, with which the amer is

increased by a fixed alkali,

and still more by a metallic oxide.

* It is well known, that, in almost all cases, where hidrogen united with other combustible substances is present with oxigen, the oxigen attacks the hidrogen preferably to the others.

† Containing perhaps a little oxigen and nitrogen.

combined,

combined, is more fixed. Thus the compound of potash with amer detonates more loudly than that of ammonia, and less than that of oxide of lead. But there are various circumstances capable of modifying the strength of the detonation: 1st, the quantity of amer united with the base: 2d, the force with which they are combined: and 3d, the nature of the base. Thus, for instance, the metallic oxides that are easily reducible form compounds, that detonate more feebly than those that are difficult of reduction.

Circumstances that modify the detonation.

24. The solution of amer precipitates isinglass: but the precipitate is soluble in an excess of gelatine, and in all the acids. This property I shall notice in a subsequent paper.

Amer precipitates gelatine.

§ III. Art. 2. *Of the Volatile Acid.*

25. The orange-coloured matter separated from the resin by boiling water (10) was little soluble in cold water. Boiling water separated some resin from it; and the crystals, that fell down on cooling, were fawn-coloured. These crystals were composed of *volatile acid*, *resin*, and a small quantity of amer. I purified it in the following manner. I dissolved 5 gram. [77 grs.] in hot water, added in five portions as many grammes of carbonate of lead, boiled and filtered. On the paper was left a yellow powder, consisting of resin and a portion of volatile alkali combined with oxide of lead. To the filtered liquor I added sulphuric acid; and the oxide of lead (with which the volatile acid had combined, forming with it a soluble compound with excess of acid) fell down in the state of sulphate. The liquor I filtered still boiling, evaporated, and obtained by refrigeration white acicular crystals, united by their extremities in the form of stars. Having left them to drain, I redissolved them in boiling water, and thus separated a small quantity of resin; when the crystals obtained by cooling were of a fine white like wax. To obtain these crystals in all their whiteness, care must be taken not to dry them on paper that contains carbonate of lime, or oxide of iron, as this would turn them yellow, or reddish.

The orange-coloured matter a compound.

The volatile acid purified.

On boiling down the mother water, that had yielded the crystals of volatile acid, more crystals of volatile acid were deposited

More obtained from the mother water.

deposited, which were tinged yellow by a little resin; and afterward a *substance of an oily appearance*, which was composed of volatile acid, amer and resin.

Properties of this substance.

26. The crystals of volatile alkali have a taste slightly acid, bitter, and astringent.

Thrown on redhot iron, one part is volatilized, another is decomposed, and leaves a coal that fuses.

They may be sublimed in white needles, by heating them gently in a phial.

Decomposed by heat.

Heated in the glass ball already described (16), they melt; part is volatilized into the jar; and what remains in the ball grows black, and leaves a bulky coal, which is slightly fusible. Much less gas is formed than in the detonation of amer.

Gaseous products.

The gasses produced in this experiment have an empyreumatic vegetable smell. Distilled water absorbs more than three fourths, and then appears to contain nothing but *carbonic acid*, except the undecomposed portion of the volatile acid. I could not obtain any prussic acid from it by distillation. The gaseous residuum insoluble in water and potash consists of *nitrogen*. I had not enough to determine, whether it contained any hidrogen.

The acid more soluble in hot water than in cold.

27. The volatile acid is far more soluble in hot water than in cold. The solution has a very slight yellow colour. It reddens litmus paper. It does not precipitate gelatine like amer; and differs from it in giving a fine red to all the salts of iron at a maximum, but it does not change the colour of the salts at a minimum.

Nitric acid converted it into amer.

28. Nitric acid at 40° [sp. gr. 1 386] boiled on the volatile acid converted it into Welther's amer. *Muriatic* acid appeared to have no action on it.

It is soluble in solution of potash.

29. The volatile acid dissolves very well in solution of potash; or in its carbonate, if assisted by heat, and carbonic acid is evolved. On boiling down this solution, which is of an orange colour, small red acicular crystals are formed. These are much more soluble in water than the compound of amer and potassium, are much less bitter, and do not detonate, but swell when placed on a redhot iron. On exposing them to the action of heat in the glass ball, *first* a yellow vapour was disengaged, after which they melted,

The compound decomposed by heat.

without

without emitting much light. An alkaline coal remained, retaining *carbonic acid* and *prussic acid*. There was also a little undcomposed volatile acid.

The gaseous product consisted in great part of carbonic acid and nitrogen. Gaseous product.

30. Lime, barytes, or strontian water, gives the solution of volatile acid a fine yellow colour. On boiling down the compound of the acid and barytes, small orange-coloured crystals are formed by cooling, which, when exposed to heat, do not detonate, but grow red hot, and afterward leave a coal, that throws out a number of little red sparks as it burns. The acid combined with the earths,

All these compounds are decomposed by sulphuric, nitric, or muriatic acid.

The volatile acid did not appear to me to decompose the muriate or nitrate of potash like amer.

31. The volatile acid boiled with oxide of silver dissolves it; but, if the solution be boiled too long, it grows black, and the oxide of silver appears to be reduced. oxide of silver,

Assisted by heat it decomposes carbonate of lead, dissolves part of the oxide, and deposits, on cooling, small orange-coloured crystals, which melt without detonating. oxide of lead,

It dissolves red oxide of iron, and acquires a hyacinthine red colour. and oxide of iron.

All these compounds appeared to me to be acid.

§ III. Art. III. *Of the resinous Matter.*

32. The resinous matter, which had been separated from the orange-coloured matter by boiling water (10), was subjected anew to the action of this agent, till the water came off very slightly coloured. This required a considerable time. The insoluble residuum was treated repeatedly with boiling water, the resin was dissolved; and a mixture of oxalate of lime, sand, &c. remained. Purification of the resinous matter.

33. The resin was separated from the alcohol by adding water, and the liquid evaporated. The resin thus obtained was brown, very slightly bitter, and gave a faint yellow tinge to the water in which it was boiled. This water did not precipitate gelatine, and did not give a red colour to sulphate of iron, but threw down with it a slight precipitate. Its properties.

It

It did not redden litmus paper, till it was concentrated by boiling.

When the resin was thrown on a red hot iron, it emitted a fragrant smell, and left a tumid coal.

Distilled.

On distilling it in a small glass retort, I obtained, among other products, a liquid, that had a strong smell of prussic acid, and of oily ammonia.

Retains nitric acid in combination.

The resin is soluble in solution of potash, nitric acid, and alcohol. I believe, that, even after it has been well washed, it contains nitric acid in real combination, and a little volatile acid and amer. It is probable, that all the resinous matters, which are formed in treating animal or vegetable compounds with nitric acid, retain a portion of this acid in combination.

Impure part soluble in water.

34. The waters in which the resin was washed (32) were reddish, grew turbid on cooling, and let fall a resinous matter, which was a little viscous, bitter to the taste, and appeared to differ from the washed resin only in containing a larger quantity of nitric acid, volatile acid, and amer. I imagine it was by means of these only, that it dissolved in the water.

On treating this resinous matter with one fourth its weight of carbonate of lead, I obtained a solution of amer, volatile acid, and lead. The resin was not dissolved, but retained a little oxide of lead in combination with it.

If the resin be boiled with an excess of carbonate of lead, it is still acid, and still yields ammonia by distillation. This confirms me in the opinion, that it retains nitric acid.

The resin treated with nitric acid.

35. The resin, when treated repeatedly with nitric acid at 45° [sp. gr. 1·455], was dissolved into a reddish brown liquor. From this water separated a substance of the yellow colour of shamoy leather, which appeared to me to be resin combined with amer and nitric acid. Some amer, matter of an oily appearance, and a little resin, remained in solution.

Amer thus formed from it.

Though the resin, that had been employed in my experiments, contained a little amer, I have no doubt, that a certain portion was formed by the action of the nitric acid, as Messrs. Fourcroy and Vauquelin have said. What appeared to me, to prevent the total conversion of the resin into

into amer, is the combination that takes place between these two products. I have even observed, that this combination is capable, in a certain degree, of defending the volatile acid from the action of the nitric acid.

§ IV. *Reflections on the Nature of the Volatile Acid and of the Amer.*

36. If the amer be considered as a compound of nitric acid and a vegetable substance, the nature of which is yet unknown, we shall be led to regard the volatile acid as a similar compound, differing from the former only in containing less nitric acid. I am fully aware, that I cannot prove these assertions by direct experiments; but, if we compare the facts mentioned in this paper, we shall see that they have a great appearance of truth. Nature of the amer and the volatile acid.

1st. The volatile acid and its compounds, when exposed to heat, comport themselves nearly in the same manner as amer and its compounds. The different quantities of nitric acid they contain explain why the former only melt, while the latter detonate forcibly.

2d. Nitric acid boiled with the volatile acid converts it into amer.

3d. The compounds of amer have a great resemblance to those of the volatile acid. Their taste is more or less bitter, their colour a yellow, more or less deep.

4th. If it be true, that the matter combined with the nitric acid is of an oily nature, we can conceive, why the amer is more soluble in water than the volatile acid, which contains less nitric acid; why the volatile acid is more soluble in alkalis than in water; and why the compounds of amer are lighter coloured than those of the volatile acid.

If it be asked, why the volatile acid, in its decomposition by heat, gives out no nitrous gas, while the amer does; I answer, there are two reasons why the nitric acid of the former should be radically decomposed: first, because the combustible elements in the former are in larger proportion than in the amer; secondly, because the nitric acid is more strongly retained by it.

37. If these reasonings be just, the volatile acid may be termed *amer with a minimum of nitric acid;* and Welther's amer, *amer with a maximum of nitric acid.*

It

It remains for farther experiments to show, whether it be possible to separate the nitric acid from these substances, without employing the assistance of heat; and whether the amer at a minimum be not a compound of amer at a maximum, already formed, with an oily or resinous matter.

I intend to subject these substances to the action of the voltaic pile as soon as leisure will permit me.

The detonating substance not a compound of amer and nitre.

38. Mr. Welther, in his paper on amer, considered the detonating substance it forms with potash as a compound of nitrate of potash and amer. This opinion was founded on the following experiment, which is very accurate. He took crystallized amer, mixed it with nitrate of potash, evaporated, and obtained the detonating substance. But I have shown above, that the detonating substance is formed with muriate of potash, as well as with nitre; and that consequently the acid of the nitre had no influence in the production of the detonating substance.

Welther's view of it different from the author's.

This experiment shows at the same time the difference there is between the view I have taken of amer in the course of this paper, and that of Mr. Welther, in his experiments on silk. In all the experiments I have described, it may have been observed, that the amer did not yield its nitric acid to any substance, that it acted on the bases by a resulting affinity, and that consequently nitric acid was a principle necessary to its existence; while Mr. Welther considered amer as a substance *sui generis*, which became detonating only by combining with nitrate of potash.

Moretti's new acid nothing but amer.

39. The new acid, which Mr. Moretti speaks of having obtained by distilling indigo with nitric acid (7), appears to be nothing but amer at a maximum; at least the properties he ascribes to it belong to the latter compound.

VII. Analysis

VII.

Analysis of Hedge Hyssop, Gratiola Officinalis, of the Order Bignonia of Jussieu: by Mr. VAUQUELIN *.

THE experiments, of which I am about to give an account, were instituted for the purpose of ascertaining the nature of the purgative principle of hedge hyssop. Object of the experiments.

1. The expressed and filtered juice of this plant has but little colour, compared with that of many other vegetables. Its taste is acrid and bitter. It is rendered very slightly turbid, by heat, or by aqueous infusion of galls, which indicates, that it contains but a very small portion of animal matter. It has also but little acidity †. The expressed juice.

2. Subjected to distillation, it yielded a water void of taste, and in which nothing was detected by a considerable number of tests. It contains, therefore, no principle, that is volatile at the temperature of boiling water. Distilled.

3. The juice being evaporated to the consistence of an extract, and treated with alcohol, the greater part dissolved in it. That which did not, was higher coloured than the other, and insipid, or with very little taste: a proof, that this property pertains to the part soluble in alcohol. The alcoholic solution, being evaporated to dryness, left a brownish yellow substance of extreme bitterness. Extract treated with alcohol.

This substance, being treated with water, imparted to it a pretty deep brown colour, and a bitter taste; leaving a soft substance, drawing out into strings like a resin, and the taste of which, at first sweetish, was afterward extraordinarily bitter. Though this substance appeared to be insoluble in water, yet it dissolves in a large quantity of this fluid when heated. Alcoholic extract treated with water.

The alcohol therefore had dissolved two substances from the extract of hedge hyssop, a resin, and a matter soluble Two substances extracted by the alcohol.

* Ann. de Chim. Vol. LXXII, p. 191.

† In plants that contain an acrid principle there is generally little or no animal matter; as if these substances were incompatible, or the circumstances favourable to the formation of the one were not suitable for the formation of the other.

in

in water. Beside the bitter taste of the latter, it has considerably pungency, owing to the salts it contains, the nature of which will be made known below.

The resin made more soluble by the salts.

It appears to be these salts, that communicate to the resin the faculty of dissolving in water more abundantly: for, once divested of them, it is much less soluble in this fluid.

Action of reagents on the solution.

This resinous substance, while dissolved in water, was exposed to the action of various tests, which exhibited the following effects:

1. Oxalate of ammonia rendered the solution slightly turbid.

2. Nitrate of barytes produced no change.

3. Muriate of platina formed a small quantity of a *triple salt*.

4. Nitrate of silver threw down a very copious yellow precipitate, part of which was soluble in nitric acid, and the remainder had all the appearance of muriate of silver.

5. With acetate of lead it gave a brownish precipitate, completely soluble in nitric acid.

6. Litmus paper was pretty strongly reddened by it.

Calcined.

7. Part being evaporated, and calcined in a platina crucible, exhaled a very acrid and pungent matter; after which it was converted into a very bulky coal, with a taste somewhat alkaline.

The coal lixiviated.

The lixivium of this coal, being evaporated, yielded crystals, that tasted like muriate of soda. These crystals, when treated with dilute sulphuric acid, effervesced briskly; which proves, that they were mixed with an alkaline carbonate. The solution of these crystals, evaporated to dryness, calcined, and redissolved in water, yielded sulphate of soda, mixed with a small quantity of sulphate of potash.

Salts.

These results prove, that hedge hyssop contains muriate of soda, and another salt with base of potash, the acid of which is of the vegetable kind, since it was destroyed by heat, and leaves carbonic acid in its stead.

It is to be presumed, that this acid is the malic, or the acetic; for the salt composed of it was dissolved by alcohol, and its solution, even when greatly concentrated, afforded no nitrate of potash: the only salt besides, that would have dissolved in alcohol, and been decomposed by heat.

The

The matter insoluble in alcohol, that has been mentioned above, had no taste, and was redissolved entirely by water, to which it gave a viscous consistence, as a gum would have done.

Matter insoluble in alcohol examined.

To satisfy myself whether it were really a gum, I digested it in hot nitric acid, which soon took away its colour, and dissolved it. The solution only remained of a light yellow.

When thus treated by nitric acid, it yielded a white flocculent substance, insoluble in water, which I took at first for mucous acid; but farther experiments led me to consider it as a mixture of this acid and oxalate of lime.

Treated with nitric acid.

To obtain this white powder separate, I decanted the supernatant yellow liquid, and afterward washed the residuum with small portions of cold water, till it was very white; when I separated it by the filter, and dried it.

White powder separated.

This powder had a slight acid taste; and, being diluted with a little water on litmus paper, it reddened it perceptibly. Placed on a burning coal, it swelled up, grew black, and emitted a smoke that smelt exactly like that of sugar treated in the same manner. In ammonia it appeared at first to dissolve; but soon after a flocculent substance formed in the liquid.

Its properties.

Having filtered the ammoniacal solution, I added a few drops of nitric acid, to saturate the alkali; and see whether the mucous acid, being little soluble, would not fall down: but the mixture remained perfectly clear. To the same solution I added lime-water, till the slight excess of nitric acid it contained was saturated; but no precipitation took place. Lastly, I mixed a pretty large quantity of alcohol with the ammoniacal solution, which produced in it no change.

From these experiments it appears, that the ammonia, with which I treated the white powder in question, dissolved no oxalic acid; since lime-water, added to it, did not render it turbid: and also, that it dissolved no mucous acid, since I could not cause any to reappear.

Yet it must have dissolved something; for the white powder was reduced to a very small quantity: and I am inclined to believe, both from the external appearance of the substance, and from the smell of burnt sugar it emitted when

Mucous acid.

when placed on a live coal, that it could be nothing but the mucous acid, notwithstanding I found it impossible to demonstrate it.

Oxalate of lime. As to the substance that was not dissolved by the ammonia, I satisfied myself by various experiments, that it was oxalate of lime.

Malate of lime. The presence of lime in this white powder indicates, that the alcohol had precipitated with the gum some malate of lime, which in fact is not soluble in that liquid. The yellow liquor decanted from the white powder contained also oxalate of lime in solution, free oxalic acid, and yellow bitter matter: for ammonia threw down from it a white granular precipitate; the filtered liquor was afterward rendered turbid by lime-water; and the solution still remained yellow and bitter.

The mucous matter. The mucous matter of hedge hyssop, which was separated from the bitter principle by means of alcohol, as said above, contained therefore lime in combination with an acid; and probably a small quantity of vegeto-animal matter, which formed the yellow bitter matter by the alteration it underwent in the nitric acid.

The green resin The green resin of hedge hyssop exhibited nothing peculiar. Like that of other vegetables, it is soluble in alcohol, in alkalis, and in fats.

Soluble principles of hedge hyssop. The experiments I have made on hedge hyssop, the chief of which I have here related, show, that this plant contains the following soluble principles, which are consequently found in its expressed juice: 1, a gummy matter of a brown colour: 2, a resinous matter; which differs however from most resins in being soluble in a large quantity of water, particularly when heated; much more soluble in alcohol than in water, and of an extremely bitter taste: 3, a small quantity of animal matter: 4, muriate of soda in pretty large quantity: and, 5, a salt with base of potash, which I suspect to be a malate. I detected the existence of this salt by means of solution of platina, and simple sulphate of alumine.

Solubility of the resin. It appears, that the solubility of the resin is increased by the presence of the gummy matter, and of the salts; for, when it is freed from these, it can no longer be dissolved in water,

water in so large proportion as it exists in the juice of the plant.

Its properties. The consistence of this sort of resinoid is that of a soft paste: but if it be exposed some time to the open air, it dries, and becomes friable. Its extraordinarily bitter taste has much resemblance to that of colocynth, though the plant that furnishes it is not of the same family: it differs from it however by a saccharine taste preceding the bitter.

Solid substance of the plant examined. After having expressed the juice of the hedge hyssop, and exhausted the magma by water and alcohol, I left it three days in diluted nitric acid. I then pressed the acid strongly through a cloth, washed the magma with water, and added ammonia to the united liquors, in which it formed a flocculent yellow precipitate. As this precipitate showed some traces of vegetable matter, I calcined it lightly; after which it dissolved with effervescence in muriatic acid, and from the solution ammonia threw down a yellow precipitate which consisted of phosphate of lime, and oxide of iron. It also yielded, on the addition of oxalic acid, a certain quantity of oxalate of lime.

The magma of the hedge-hyssop still contained oxalate of lime, phosphate of lime, and iron, which also was probably united with phosphoric acid.

Lastly, the magma, having been burned, left ashes consisting for the most part of silex, with a little calcareous earth and iron.

The active principle. From what has been said there appears no doubt, that the active and cathartic principle of hedge hyssop is the substance soluble in alcohol, which I have called a resinoid; since it is the only one in it, that has any taste. Its solubility in water, which is increased by the gum and the salts that accompany it, explain why the infusion, and still more the decoction of the plant, are purgative, and even drastic.

Formerly prohibited, for its violence. The violent action of hedge hyssop on the animal economy has long been known to physicians; and this no doubt is the reason, why the sale of this plant has formerly been prohibited. This prudent measure, which has fallen into disuse, ought to be revived; for accidents frequently happen

from the ignorance of herbalists, or of those who use this plant, respecting its virtues.

Instance of its danger.

To mention only one recent instance: a man complaining of a pain in the loins, where one of those good women were present, who act gratuitously as physicians, and have a remedy for every disease; she advised him to have recourse to clysters of a decoction of hedge hyssop. Unfortunately he too readily followed the prescription; for, a few moments after the remedy was administered, he was seized with violent griping pains, and these were succeeded by evacuations of blood, which continued for more than a week; and, but for the assistance of a skilful physician, who ought to have been consulted before, he would probably have lost his life.

VIII.

On the Causes which influence the Direction of the Growth of Roots. By T. A. Knight, *Esq. F. R. S. In a Letter to the Right Hon. Sir* Joseph Banks, *Bart. K. B. P. R. S**.

Growth of radicles.

I HAVE shown, in a former communication, the effects of centrifugal force upon germinating seeds; from which I have inferred, that the radicles are made to descend towards the earth, and the germes, or elongated plumules, to take the opposite direction, by the influence of gravitation; and I believe the facts I have stated to be sufficient to support the inferences I have drawn †. But the fibrous roots of plants, being much less succulent, though not uninfluenced in the directions they take by gravitation, are, to a great extent, obedient to other laws, and are generally found to extend themselves most rapidly, and to the greatest length, in whatever direction the soil is most favourable: whence many naturalists have been disposed to believe, that these are guided by some degrees of feeling and perception, analogous to those of animal life.

Fibrous roots obedient to other laws;

whence supposed to have perception.

* Phil. Trans. for 1811, p. 209.

† Phil. Trans. 1806, page 5: or Journal, vol. xiv, p. 409.

I shall

I shall proceed to state some of the facts, upon which this hypothesis has been founded; and others, which have occurred in the course of my own experience, and which are favourable to it; after which I shall endeavour to trace the effects observed to the operation of different causes.

Facts on which this opinion is founded.

When a tree, which requires much moisture, has sprung up, or been planted, in a dry soil, in the vicinity of water, it has been observed, that much the largest portion of its roots has been directed towards the water; and that when a tree of a different species, and which requires a dry soil, has been placed in a similar situation, it has appeared, in the direction given to its roots, to have avoided the water and moist soil.

Roots tending to or from water.

A tree growing upon a wall, at some distance from the ground, and consequently ill supplied with food and water, has also been observed to adapt its habits to its situation, and to make very singular and well directed efforts to reach the soil beneath, by means of its roots *. During the period in which it is making such efforts, little addition is made to its branches, and almost the whole powers of the plant appear to be directed to the growth of one or more of its principal roots. To these much is in consequence annually added, and they proceed perpendicularly towards the earth, unless made to deviate by some opposing body: and as soon as the roots have attached themselves to the soil, the branches grow with vigour and rapidity, and the plant assumes the ordinary habits of its species.

Roots from a tree on a wall.

Du Hamel caused two trenches to be made so as to intersect each other at right angles, and a tree to be planted at the point of intersection; and taking up this tree some years afterward, he found that the roots had almost wholly confined themselves to the trenches, in which the soil of the former surface must have been buried.

Tree planted in the intersection of two trenches.

A trench, which was twenty feet long, six wide, and about two deep, was prepared in my garden, in the bottom of which trench was placed a layer, about six inches deep, of very rich mould, incorporated with much fresh vegetable matter.

Carrots and parsneps sown on a poor soil with a rich substratum:

* Smith's Introduction to Botany.

matter. This was covered, eighteen inches deep, with light and poor loam; and upon the bed thus formed seeds of the common carrot (*daucus carota*) and parsnep (*pastinaca sativa*) were sowed. The plants grew feebly till near the end of the summer, when they assumed a very luxuriant growth, grew rapidly till late in the autumn, and till their leaves were injured by frost. The roots were then examined, and were found of an extraordinary length, and in form almost perfectly cylindrical, having scarcely emitted any lateral fibrous roots into the poor soil, while the rich mould beneath was filled with them.

Others in a rich soil with a poor substratum.

In another experiment of the same season, the preceding process was reversed, the rich soil being placed upon the surface, and the poor beneath. The plants here grew very luxuriantly, and acquired a considerable size early in the summer; and when the roots were taken up in the autumn, they were found to have assumed very different forms. The greater part had divided into two or more unequal ramifications, very near the surface of the ground, and those which were not thus divided tapered rapidly to a point at the surface of the poor soil, into which few of their fibrous roots had entered.

Plants growing so as to select either soil.

In other experiments seeds of almost all the common esculent plants of a garden were so placed, that the young plants had an opportunity of selecting either rich or poor soil; which was disposed, in almost every possible way, within their reach; and I always found abundant fibrous roots in the rich soil, and comparatively few in the poor.

Beans placed so as to be exposed to the air beneath them and to earth above them.

The following experiment afforded the most remarkable result, and one the least favourable to the hypothesis, which I have advanced in a former paper *, and to the conclusion which I shall now endeavour to support; and therefore I think it necessary to describe it very minutely. Some seeds of the common bean (*vicia faba*), the plant with which many former experiments were made, were placed upon the surface of the mould in garden pots, in rows which were about four inches distant from each other. A grate, formed of slender bars of wood, was then adapted to the surface

* Phil. Trans. 1806, page 1: or Journal, vol. xiv, p. 403.

of

of each pot, so as to prevent both the mould and the seeds falling out, in whatever position the pots might be placed: and the bars were so disposed, as not at all to interfere with the radicles of the seeds, when protruding. The pots were then directly inverted: and the seeds were consequently placed beneath the mould; but each seed was so far depressed into the mould, as to be about half covered: by which means each radicle, when first emitted, was in contact with the mould above, and the air below. Water was then introduced through the bottom of the inverted pot, in sufficient quantity to keep the mould moderately moist; and, the pots being suspended from the roof of a forcing house, the seeds soon vegetated.

The radicles extended horizontally, and shot up fibres into the earth.

In former experiments *, wherever the seeds were placed to vegetate at rest, the radicles descended perpendicularly downward, in whatever direction they were first protruded; but under the preceding circumstances they extended horizontally along the surface of the mould, and in contact with it; and in a few days emitted many fibrous roots upward into it: just as they would have done, if guided by the instinctive faculties and passions of animal life; and as I concluded before I made the experiment that they would do, under the guidance of much more simple laws, the mode of operating of which I shall endeavour to explain.

Ascending sap first set in motion by the stems and branches,

Whatever be the machinery, by which the sap of trees is raised to the extremities of their branches, it is obvious, that this machinery is first put into action by the stems and branches, and not by the roots; for the graft or bud, whenever it has become fully united to the stock, wholly regulates the season and temperature, in which the sap is to be put in motion, in perfect independence of the habits of the stock; whether these be late or early. If all the branches of a tree, exclusive of one, be much shaded by contiguous trees †, or other objects, the branch which is exposed to the light attracts to itself a large portion of the ascending sap, which it employs in the formation of leaves and

and the quantity replaced by their ability to employ it, to which light is conducive.

* Phil. Trans. 1806, p. 1: or Journal, vol. xiv, p. 409.

† Phil. Trans. 1805 and 1809: or Journal, vol. xii, pp. 233, 308; vol. xxv, p. 118.

vigorous

vigorous annual shoots, while the shaded branches become languid and unhealthy. The motion of the ascending current of sap appears therefore to be regulated by the ability to employ it in the trunk and branches of the tree; and this current passes up through the alburnum, from which substance the buds and leaves spring. But the sap, which gives existence to, and feeds the root, descends through the bark *: and if the operation of light give ability to the exposed branch to attract and employ the ascending or alburnous current of sap, it appears not improbable, that the operation of proper food and moisture in the soil, upon the bark of the root, may give ability to that organ to attract and employ the descending, or cortical current of sap; and if this be the case, an easy explanation of all the preceding phenomena immediately presents itself.

Proper food enables the root to attract and employ the descending sap.

Descent of roots from a tree on a wall accounted for.

A tree growing upon a wall, and unconnected with the earth, will almost of necessity grow slowly, and as it must be scantily supplyed with moisture during the summer, it will rarely produce any other leaves, than those which the buds contained, which were formed in the preceding year. Some of the roots of a tree, thus circumstanced, will be less well supplied with moisture than others, and these will be first affected by drought: their points will in consequence become rigid and inexpansible, and they will thence generally cease to elongate at an early period of the summer. The descending current of sap will be then employed in promoting the growth and elongation of those roots only, which are more favourably situate, and these comparatively with other parts of the tree, will grow rapidly †. Gravitation will direct these roots perpendicularly downward, and the tree will appear to have adopted the wisest and best plan of connecting itself with the ground: and it will really have employed the readiest means of doing so, as effectively as it could have done, if it had possessed all the feelings and instinctive passions and powers of animal life. The

* Phil. Trans. 1809, p. 169: or Journal, vol. xxv, p. 119.

† We do not find here, however, "the proper food and moisture," to "give ability to the root to attract and employ the descending or cortical current of sap." C.

subsequent

subsequent vigorous growth of such a tree is the natural consequence of an improved and more extensive pasture.

When the seeds of the carrot and parsnep, in the experiments I have stated, were placed in a poor superficial soil, but which permitted the roots of the plants to pass readily through it, these were conducted downward by gravitation; while the plants grew feebly, because they received but little nutriment. The roots were in a situation analogous to that of the stems of trees in a crowded forest; and when the leading fibres of the roots came into contact with the rich mould, they acquired a situation correspondent to that of the leading branches of such trees, which are alone exposed to the light. The form of the roots of the plants was consequently long, slender, and cylindrical, like the stems of such trees. The roots of the one required the actual contact of proper soil and nutriment; and the branches of the other required the actual contact of light, to promote their growth.

Growth of the carrot and parsnep seeds explained in the first case,

When, on the contrary, the seeds of the preceding species of plants were placed in a rich superficial soil, their situation was analogous to that of a tree fully exposed, on every side, to the light; the branches of which would be extended, in every direction, immediately above the surface of the ground: and as the fibrous roots of the plants came into contact with the subsoil, which was not well calculated to promote their growth, their situation became analogous to that of shaded branches; and they consequently ceased to extend downwards. The fibrous roots of a tree, under similar circumstances, would have extended along the lower surface of the favourable soil; but after these roots had much increased in bulk, they would be found partly compressed into the subsoil, however poor and unfavourable, provided it contained no ingredients actually noxious.

and in the second.

In obedience to similar laws, the roots of an aquatic tree will not extend freely in dry soil, nor those of a tree which requires but little moisture in a wet soil; and on this account the roots of the one will appear to have sought, and those of the other to have avoided, the contiguous water; though both, in the first period of their growth, pointed their roots alike in every direction.

Growth of trees requiring much or little moisture.

When

Explanation of the growth of the beans, p. 372.

When the seeds of the bean, in the experiments I have described, were placed to vegetate beneath the mould of an inverted pot, a sufficient quantity of moisture was afforded by the mould, to occasion the protrusion of the radicles: but as soon as the under points of these had penetrated through the seed-coats, their surfaces were necessarily exposed to dry air, and were consequently rendered rigid and inexpansible; while their upper surfaces, being in contact with the moist mould, remained soft and expansible. If both the upper and lower surfaces of the radicles, at their points, had been equally well supplied with moisture, gravitation would have attracted the sap to the lower sides, where new matter would have been added; and the radicles would have extended perpendicularly downward, as in former experiments; but the influence of gravitation was, to a great extent, counteracted by the effects of drought upon the lower sides of the radicles, nearly as it was counteracted by centrifugal force, when made to act horizontally *.

As soon as the radicles had acquired sufficient age and maturity, efforts were made by them to emit fibrous roots; when want of proper moisture on the lower sides prevented their being protruded, in any other *direction, except upwards.* In this direction therefore they were alone emitted, (as I was confident that they would before I began the experiment) and having found proper food and moisture in the pots, they extended themselves upward through more than half the mould, which these contained.

The experiment repeated with additional moisture.

This experiment was repeated, and water was so constantly and abundantly given, that every part of the radicles was kept equally wet; and they then became *perfectly* obedient to gravitation, without being at all influenced by the mould above them.

Sulphates of alum, iron, and copper, did not check the growth of roots but by actual contact.

In other experiments, pieces of alum and of the sulphates of iron and copper were placed at small distances perpendicularly beneath the radicles of germinating seeds, of different species, to afford an opportunity of observing, whether any efforts would be made by them to avoid poisons; but they did not appear to be at all influenced,

* Phil. Trans. 1806, p. 6: or Journal, vol. xiv, p. 111.

except

except by actual contact of the injurious substances. The growth of their fibrous lateral roots was, however, obviously accelerated, when their points approached any considerable quantity of decomposing vegetable or animal matter: and when the growth of the roots was retarded by want of moisture, the contiguity of water, in the adjoining mould, though not apparently in actual contact with them, operated beneficially: but I had reason to suspect, that the growth of roots was, under these circumstances, promoted by actual contact with the detached and fugitive particles of the decomposing body, and of the evaporating water.

Food and moisture also require contact to promote growth.

The growth and forms assumed by the roots of trees, of every species, are, to a great extent, dependent upon the quantity of motion, which their stems and branches receive from winds; for the effects of motion upon the growth of the root, and of the trunk and branches, which I have described in a former memoir, are perfectly similar*. Whatever part of a root is moved and bent by winds, or other causes, an increased deposition of alburnous matter upon that part soon takes place; and consequently the roots, which immediately adjoin the trunk of an insulated tree, in an exposed situation, become strong and rigid; while they diminish rapidly in bulk, as they recede from the trunk, and descend into the ground. By this sudden diminution of the bulk of the roots, the passage of the descending sap, through their bark, is obstructed; and it in consequence generates, and passes into many lateral roots; and these, if the tree be still much agitated by winds, assume a similar form, and consequently divide into many others. A kind of net-work, composed of thick and strong roots, is thus formed, and the tree is secured from the dangers, to which its situation would otherwise expose it.

Growth and form of roots modified by the motion impressed on the tree by winds.

Hence trees most exposed are rendered more secure;

In a sheltered valley, on the contrary, where a tree is surrounded and protected by others, and is rarely agitated by winds, the roots grow long and slender, like the stem and branches, and comparatively much less of the circulating fluid is expended in the deposition of alburnum beneath

while sheltered trees have only long and slender roots.

* Phil. Trans. 1803, p. 7.

neath the ground, and hence it not unfrequently happens, that a tree, in the most sheltered part of a valley, is uprooted; while the exposed and insulated tree, upon the adjoining mountain, remains uninjured by the fury of the storm.

Plants void of sensation and passion analogous to those of animal life.

In all the preceding arrangement, the wisdom of nature, and the admirable simplicity of the means it employs, are conspicuously displayed; but I am wholly unable to trace the existence of any thing like sensation or intellect in the plants: and I therefore venture to conclude, that their roots are influenced by the immediate operation and contact of surrounding bodies, and not by any degrees of sensation and passion analogous to those of animal life; and I reject the latter hypothesis, not only because it is founded upon assumptions, which cannot be granted, but because it is insufficient to explain the preceding phenomena, unless seedling plants be admitted to possess more extensive intellectual powers, than are given to the offspring of the most acute animal. A young wild-duck or partridge, when it first sees the insect upon which nature intends it to feed, instinctively pursues and catches it; but nature has given to the young bird an appropriate *organization*. The plant, on the contrary, if it could feel and perceive the objects of its wants, and will the possession of them, has still to contrive and form the organ by which these are to be approached. The writers, who have contended for the existence of sensation in plants, appear to have been sensible of the preceding and other obstacles, and have all betrayed the weakness of their hypothesis, in adducing a few facts only which are favourable to it, and waving wholly the investigation of all others.

In the description of the preceding experiments, I fear that I have been tediously minute; but, as I have selected a few facts only from a great number, which I could have adduced, I was anxious to give as accurate and distinct a view of those I stated, as possible.

I am, dear Sir,
with great respect,
sincerely yours,
THOS. AND. KNIGHT.

Downton, Jan. 15, 1811.

IX. *On*

IX.

On the mucilaginous State of Distilled Waters: by Mr. BUCHOLZ *.

Distilled waters apt to spoil.

IT is well known, that distilled waters spoil more or less quickly. They become mucilaginous, deposit a flocculent sediment, lose their smell and taste, and often acquire a fetid smell and putrid taste; all which appears to take place most frequently in waters destitute of essential oil.

Circumstances favourable to this.

It is known too, that this change proceeds more rapidly in proportion as the water contains but little oil; and if the distillation have been performed hastily, the flocculent sediment forms presently after, as in elder-flower water, linden water, &c.

They spoil in close and in open vessels.

Distilled waters spoil equally in open vessels, and in vessels closely stopped: but the change takes place more speedily in very close vessels.

We have two questions then to solve: What is the cause of this alteration? And what are the means of obviating it?

The oil supposed to be decomposed.

As waters distilled with the greatest care undergo this change, it may be suspected, that the oil is decomposed, and converted into mucilage †. Bauhoff's experiments tend to support this opinion. He dissolved in common distilled water, essential oil of peppermint, of fennel, of lemons, and of valerian. These waters, which were perfectly limpid, were kept in closely stopped bottles at the common temperature; and in a few weeks they became turbid, let fall a flocculent, mucilaginous sediment, and lost their smell.

But they may be fetid without the oil being destroyed.

A fetid smell however does not always indicate the total disappearance of the essential oil. Bauhoff examined some spoiled rosewater, that had been kept in a close vessel. The surface of this water was covered with a black pellicle,

* Ann. de Chim. vol. lxiii, p. 90. Abridged from Tromsdorff's Pharmaceutical Journal, by Mr. Vogel.

† This appears inconsistent with what is said in the first paragraph. In the experiments of Bauhoff, that follow, as the term *dissolved* is employed, no doubt sugar or mucilage was used as an intermedium for uniting the oil and water. C.

and

and its smell resembled that of sulphuretted hidrogen gas. Being left a few weeks exposed to the open air, the fetid smell vanished, and was replaced by that of roses*. Some rosewater, the putrefaction of which was very evident, recovered its smell by the addition of a little lime and iron.

It appears certain therefore, that the oils in distilled waters change their nature.

Waters, that have been distilled with too strong a heat, contain lest oil; which would seem to prove, that a part of it has undergone some sort of alteration.

Waters that contain no essential oil spoil.

But there are waters, that contain no essential oil, as those of elder-flowers, borage, nettles, &c. These waters probably carry up in distillation volatile odorant principles, which approach the nature of essential oils, and are decomposed still more easily.

The author's theory.

But how are these principles converted into mucilage? As the flocculent matter forms more commonly in well stopped bottles, than when exposed to the air, this question may be easily answered. It is well known, that essential oils exposed to the air are converted into resins. This cannot be employed to explain the phenomenon. We must suppose then, that the oil, in passing to the state of mucilage, loses a part of its hidrogen; or that the oil becomes mucilage by uniting with one of the constituent principles of water, which however appears less probable. Perhaps it may be supposed, that the nitrogen of the air combines with the oil, or with the volatile odorant principles, to form mucilage.

Green matter in distilled water.

An analysis of the flocculent matter would tend to elucidate this point. However the remark made by Priestley and Sennebier would still remain to be explained. They both observed a green matter in distilled water exposed to the

Freezing advantageous to distilled waters.

* Mr. Nachet, professor at the School of Pharmacy, long ago remarked, that distilled waters, which had been frozen, acquired a more powerful smell, and kept longer. He observed this to be particularly the case with balm, mint, and orange-flower water. *Vogel.*

The experiment of Bauhoff, given in the text, tends to confirm the opinion, that the change is *not* owing to a decomposition of the essential oil. C.

rays

rays of the sun in vessels slightly covered. Sennebier found in this substance a number of small worms.

The second question remains, that of preventing distilled waters from spoiling. To prevent this inconvenience as far as possible, they should be kept in an airy cellar, in wide-mouthed vessels, covered with a paper. Once a month the paper should be taken off to renew the air at the surface. *Best method of keeping distilled waters.*

It is advisable, to have these waters in the most concentrated state possible, so that their surface may be covered with a stratum of the volatile oil of the vegetable, which may afterward be separated by filtration. If the spoiling of distilled waters cannot wholly be prevented by these means, it may at least be deferred.

X.

A new Analysis of Ambergris: by Mr. Bucholz *.

THE author first reviews the various analyses, that have been made of ambergris, and gives a comparative table of results obtained by modern chemists, namely by Rose, Juck, Bouillon-Lagrange, and Proust. He then subjected ambergris to the following experiments. *Various analyses of ambergris.*

Water distilled from ambergris acquires a slight smell of this substance, without containing an oil. *Its habitudes with water,*

Pure alcohol dissolves ambergris entirely, except a small quantity of black pulverulent matter. It dissolves a much larger quantity, if assisted by heat; and lets none fall on cooling. The liquor is then of a reddish brown †. *alcohol,*

* Ann, de Chim, vol. lxiii, p. 95. Abridged from Trommsdorff's Pharmaceutical Journal, by Mr. Vogel.

† When ambergris is treated with a small quantity of boiling alcohol, and the liquor filtered while hot, a yellowish white grumous substance is precipitated. If Mr. Bucholz did not observe this, it was probably owing to the small quantity, on which he operated. He employed only 20 grs. of ambergris to six drachms of alcohol, which he calls a saturated tincture. *Bouillon-Legrange.*

Ether

ether,

Ether dissolves it cold, and also leaves the black matter. This solution is not precipitated either by alcohol, or by water.

potash,

Caustic potash, whether dry or dissolved in water, combines very difficultly with one part of ambergris. This sparing solubility in potash may be employed as a test, to distinguish true ambergris from spurious.

and oils.

Oil of turpentine and oil of almonds dissolve ambergris very well, if assisted by heat.

Considered as a peculiar principle.

Instead of finding ambergris to be a compound of adipocere, resin, benzoic acid, and carbonaceous matter, agreeably to the results of Mr. Bouillon-Lagrange *, the author considers it as a substance *sui generis*. In the recapitulation of his paper, he expresses himself thus: "ambergris is a peculiar compound, which is a medium between wax and resin; differing from both in the manner in which it comports itself with alkalis; and approaching the resins, in alcohol dissolving a larger quantity of it than of wax, and in having a resinous aspect when it is cooled after having been melted. The author proposes to call it the *ambry principle* †."

* See Journal, vol. vi, p. 179.

† If a drachm of ambergris be dissolved in two drachms of boiling alcohol, and the liquor filtered while hot, it lets fall on cooling that substance, which I have compared to adipocere, because it approaches it nearly in its properties. The supernatant fluid is rendered turbid by water, and reddens a weak infusion of litmus. This property is owing no doubt, as I have said in a former paper, to a small quantity of resin which it contains. I did not think it right therefore, to increase the number of new substances unnecessarily, by giving to the matter precipitated from the alcohol the name of *ambry principle*; I was satisfied with considering it as intermediate between resin and wax. As to the benzoic acid, I must confess, that I have found none in several specimens of ambergris, which I have analysed since. This leads to the suspicion, that there are manufactories of ambergris, as there are of castor.

Benzoic acid probably from sophistication.

XI. *Process*

XI.

Process for preparing pure Phosphoric Acid: by Mr. MARTRES, *Apothecary at Montauban, and Member of several Societies* *.

WHILE physicians devote their studies to the search after new means of alleviating the sufferings of mankind, it is the duty of apothecaries to second their efforts, by endeavouring to simplify or improve the preparation of medicines.

Improvements in pharmacy desirable.

Dr. Lasalle, of Montauban, having employed the phosphoric acid with success in the treatment of some diseases, I endeavoured to supply him with it very pure, in a little time, and without danger, by a process, which I have found to succeed completely: being fully convinced, that we cannot rely with entire confidence on preparations, that come to us in the ordinary way of trade.

Phosphoric acid used medicinally.

We may proceed in six different ways, to obtain phosphoric acid; but five of them are not very easy of execution, and the product is almost always contaminated with phosphorous acid.

Methods of preparing it.

The sixth, pointed out by Lavoisier, yields a pure phosphoric acid, but exposes the operator to some danger. This therefore it is desirable to obviate; which I have effected by means of an apparatus, that I shall describe before I give an account of my process.

Improved method.

The neck of a retort, placed on a sand-heat, is to be introduced into the mouth of a receiver, the second neck of this receiver into the mouth of another, and the second neck of this into a curved adopter, the mouth of which opens in a vessel of water, so as to answer the purpose of a tube of safety †.

Apparatus.

The apparatus being thus arranged, 32 gr. [494 grs.] of phosphorus are to be put into the retort, through its tubulure; and an equal weight of a mixture consisting of

Process described.

* Ann. de Chim. vol. lxxiii, p. 99.

† The atmospheric air expelled from the receivers by the nitrous vapours escapes from the mouth of the adopter, which may afterward be stopped.

equal

equal parts of concentrated nitric acid and distilled water. A tube of safety is then to be introduced into the same aperture, so that its toothed extremity shall reach the bottom of the retort. The apparatus is then to be luted, and dried.

The process is to commence with heating the sandbath till the liquor boils, and the phosphorus melts. A quantity of nitric acid is then to be poured into the funnel, sufficient to produce a level, without flowing into the retort. 8 gr. [123·5 grs.] of the same acid are then to be added; which, by their weight, force into the retort an equal quantity of the liquid, part of which still remains in the tube, without touching the phosphorus.

The phosphorus, retained at the bottom by its specific gravity, attracts the nitric acid, but, as it receives only a small quantity at a time, the combustion is slow, and effected without danger.

In proportion as the nitrous vapours in the retort diminish, a fresh portion of acid is to be poured into the funnel; and this is to be repeated, till the phosphorus is completely oxigenated.

Quantity of nitric acid required.

To effect the perfect combustion of 32 gr. [494 grs.] of phosphorus, I have employed 128 gr. [1977 grs.] of concentrated acid; or 192 gr. [2965·5 grs.] of what is commonly called fuming nitrous acid.

Results.

By operating in this manner, we obtain phosphoric acid, mixed with nitrous gas, and a quantity of superfluous liquid, from which it is to be freed by evaporation. This process takes more time with a retort, than it would with a matrass; but the operator is not exposed to inhale the nitrous gas. The liquid residuum should have the consistence of a thin sirup, and leave streaks on the glass, as milk or oil would do.

If the process I have just described produce pure phosphoric acid; if my simple and ready apparatus secure the operator from the nitrous fumes, and the accidents that might be occasioned by the explosion of the vessels; this apparatus and this process will no doubt be adopted, and perhaps not be thought uninteresting to those, who are engaged in chemistry.

INDEX.

I

houses

Rawlisn,

Thomson,

ERRATA.

74 Note, *for* xxvii, p 192, *read* lxxiii, p. 102.
127 *line* 5 *for* H. J. B. *read* H. T. B.
156 Note, *for* lxii, *read* lxxii.

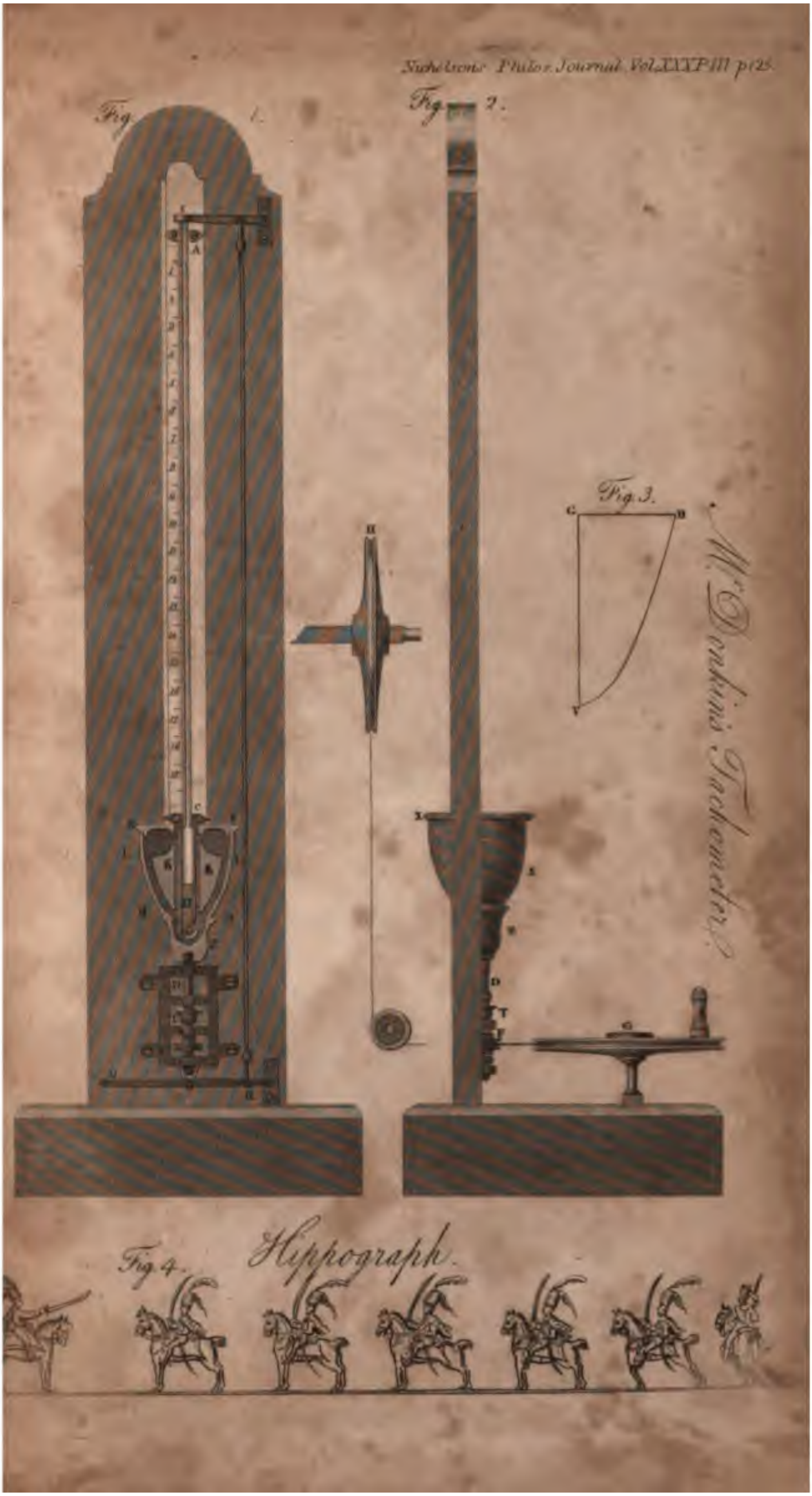
END OF THE THIRTIETH VOLUME.

Stratford, Printer, Crown Court, Temple Bar.

Hairs on Plants.

THE NEW
PUBLIC LI
ASTOR, LENO
TILDEN FOUN

Hairs on Plants & Cryptogamia.


Nicholson's Philos. Journal Vol. XXXVIII p.125
Fig. 1.
Fig. 2.
Fig. 3.
Mr Donkin's Tachometer.
Fig. 4.
Hippograph.

Mr. G. Marshall's New Window Sash.

Fig. 3.

Mr. Ross's Machine for separating Iron filings from those of Brass.

Fig. 2.

Fig. 1

Fig. 1. Fig. 2.

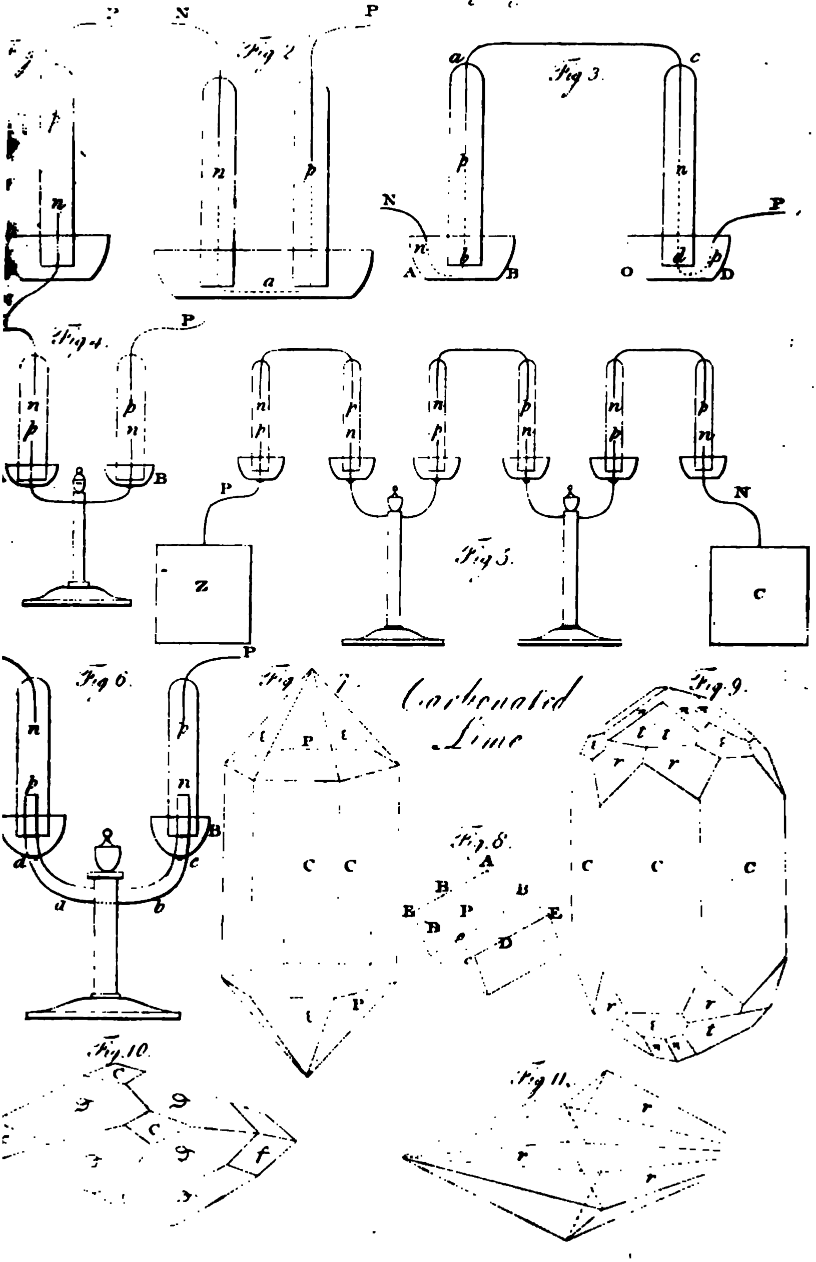
Fig. 3.

Mr Donkin's Tachometer.

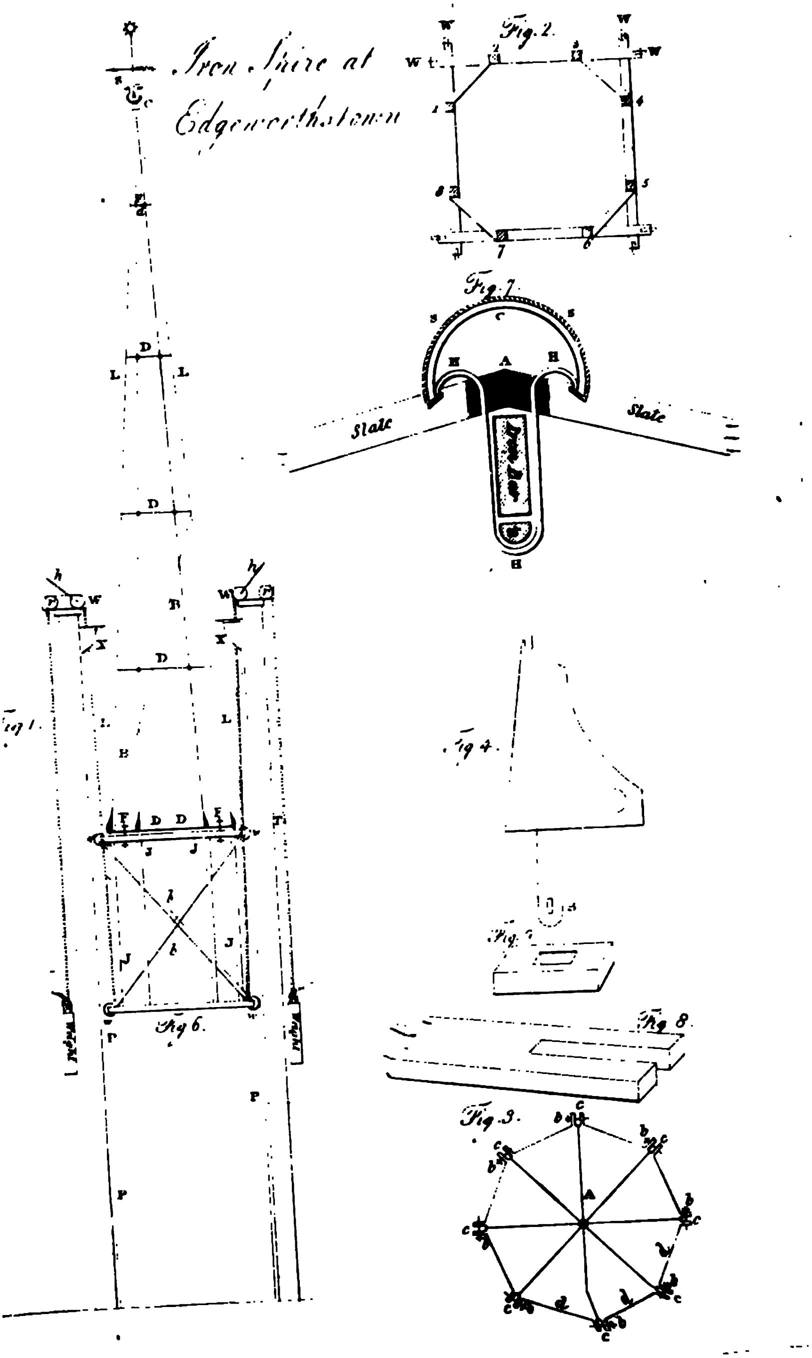
Hippograph.

Fig. 4.

Decomposition of Water by Galvanism.



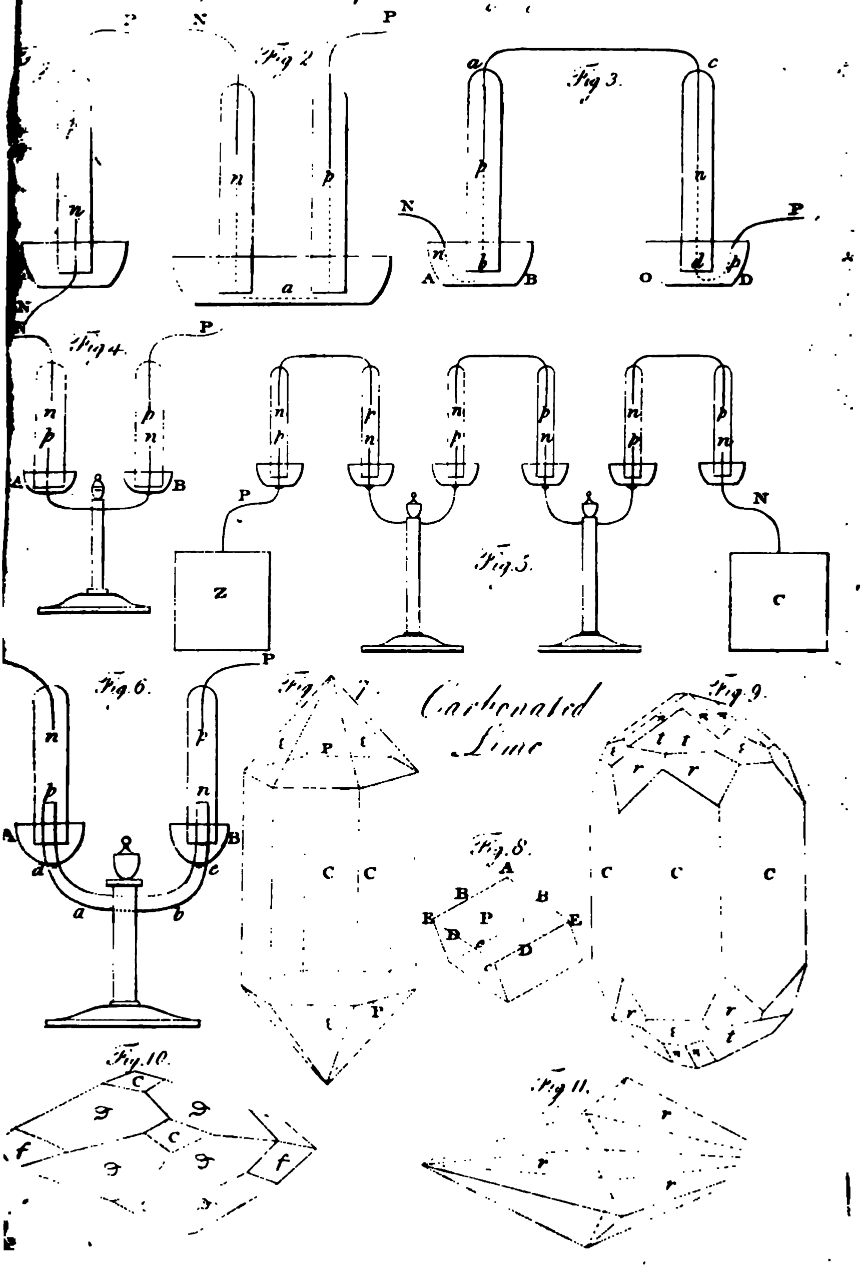
Carbonated Lime


Iron Spire at Edgeworthstown
Fig. 1.
Fig. 2.
Fig. 3.
Fig. 4.
Fig. 5.
Fig. 6.
Fig. 7.
Fig. 8.
Slate
Slate

THE NEW YORK
PUBLIC LIBRARY

ASTOR, LENOX AND
TILDEN FOUNDATIONS

Decomposition of Water by Galvanism



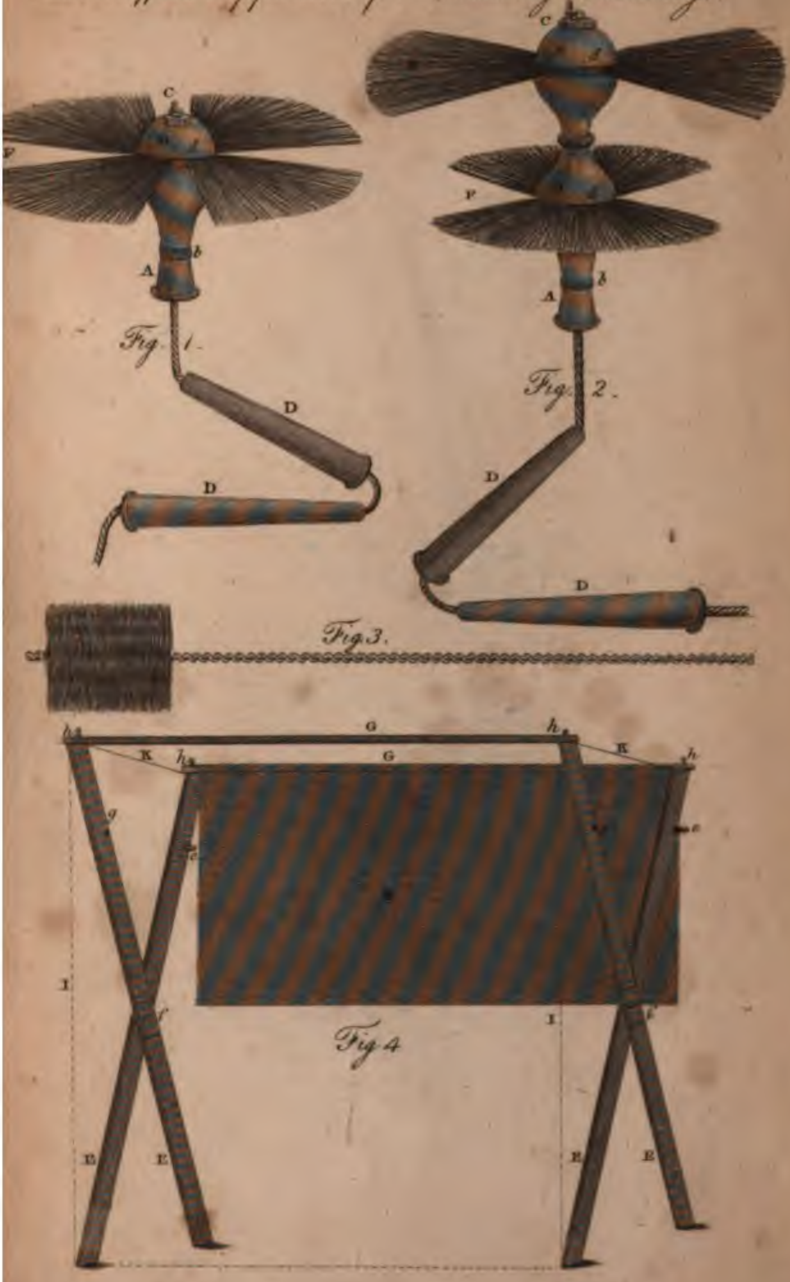
THE NEW YORK
PUBLIC LIBRARY

ASTOR, LENOX AND
TILDEN FOUNDATIONS

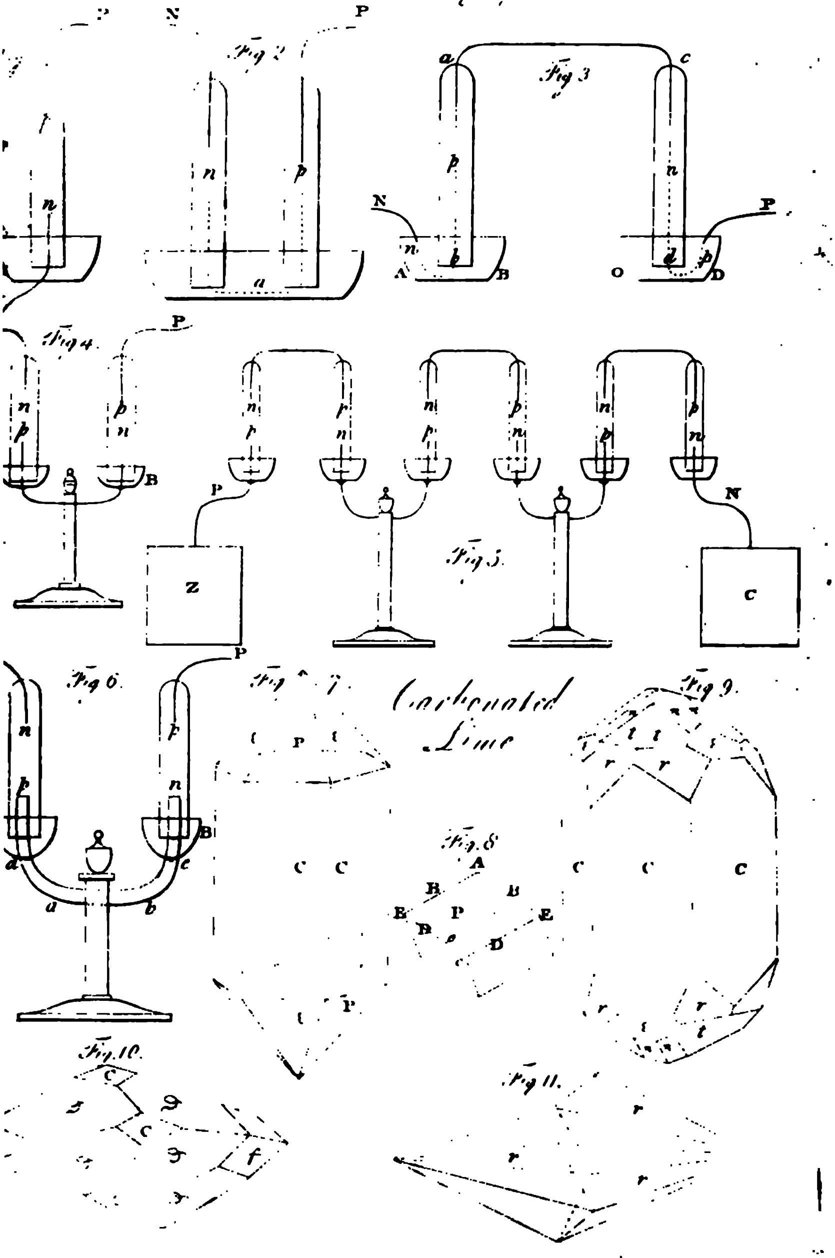

Mr. Salisbury's method of Training Plants.
Mr. Lester's Potatoe Washer
Fig. 1.
Fig. 3.
Fig. 3.
Fig. 2.
A
A
E
E
F
F
D
K
H
D
H
1
2
3
4
5
6
6
3 Feet
2 Feet
b
b
b
Section of Mr. Salisbury's Beds for young Plants.

THE NEW Y...
...BLIC LIBRARY
...NOX AND
...NDATIONS

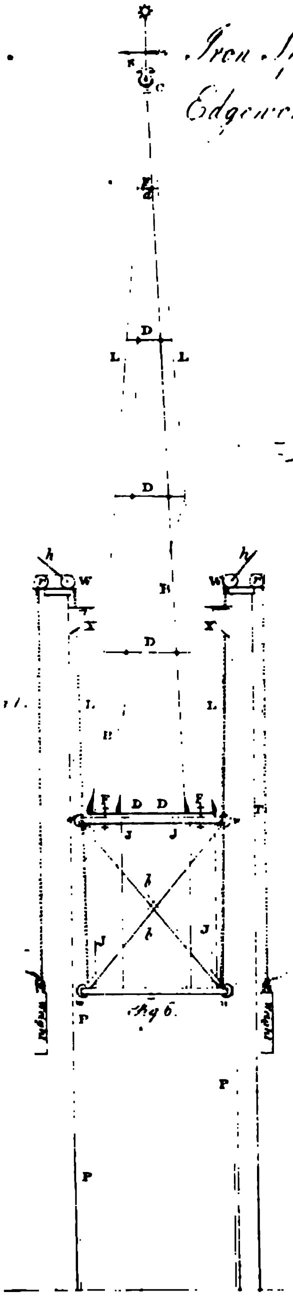
The Sheffield Apparatus for Cleaning Chimneys.

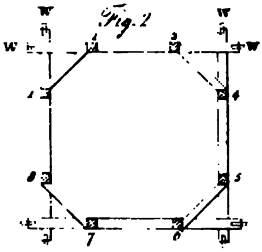


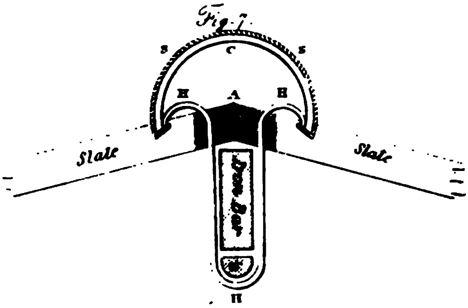
Decomposition of Water by Galvanism.

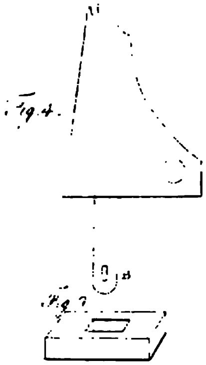


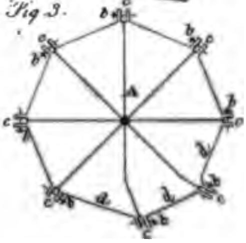
Iron Spire at Edgeworthstown.











THE NEW YORK
PUBLIC LIBRARY

ASTOR, LENOX AND
TILDEN FOUNDATIONS

Section of Mr. Salisbury's Beds for young Plants.

THE NEW YORK
PUBLIC LIBRARY
LENOX AND
FOUNDATIONS

Section of Mr. Salisbury's Beds for young Plants.

THE NEW YORK
PUBLIC LIBRARY

ASTOR, LENOX AND
TILDEN FOUNDATIONS

The Sheffield Apparatus for Cleaning Chimneys.